PERFORMING SCIENCE, PRODUCING NATION: ARCHAEOLOGY AND THE STATE IN POSTCOLONIAL INDIA

A DISSERTATION

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I certify that I have read this dissertation and that in my opinion it is fully adequate, in scope and quality, as dissertation for the degree of Doctor of Philosophy.

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Approved for the University Committee on Graduate Studies

Abstract

This dissertation is an ethnography of scientific archaeology as practiced, performed, and articulated by the postcolonial state in India. It critically focuses on bureaucratic governmentality in the production of archaeology as an authoritative cultural and scientific discourse. The results are based on more than two years of multi-sited fieldwork between June 2003 and September 2005 in India, which included archival research, and ethnographic investigations of Archaeological Survey of India's (ASI) excavations of Harappan sites in western India. The dissertation concentrates on the micro-processes of statist archaeology in the construction of objective evidence at the particularistic location of the excavation site. It provides an ethnographic account of the everyday practices of ASI archaeology through which scientific methods collapse with administrative bureaucracy in the construction of knowledge at the excavation site. Located at the intersection of the sociology of science, anthropology of the state and theoretical archaeology, the research is driven by the analyses of five respective areas: i) spatial formation of the archaeological field and technologies of governmentality through which the ASI transforms landscape into epistemic site; ii) professional subjectivity of postcolonial bureaucratic hierarchy and the institutional structure of postcolonial ASI; iii) scientific discovery of material culture and its transformation into empirical evidence iv) the conception of research problems and categorization of cultural deposits within the temporal and stratigraphic micro-context of the layer in the excavation trench; v) representational practices of the archaeological excavation and how classification and typologies produced at the excavation site are transformed into authoritative knowledge. I argue that science and statist ideologies collapse in the practice of postcolonial archaeology and it is not merely the manipulation of data by ideological agendas that gives rise to nationalist archaeology. My research demonstrates that postcolonial scientific archaeology is itself an ideological practice; the boundaries between construction of scientific evidence and its ideological manipulation are non-existent and, in fact, present a post-facto perspective on the process of archaeological knowledge production.

Acknowledgements

The origin of this dissertation is located in one hot summer afternoon when I arrived at Deccan College Post Graduate and Research Institute, Pune, in April 1994 from Bombay. This was an academic reconnaissance trip. I actually wanted to enroll myself for a Master's degree at the Anthropology Department of Pune University after having just completed a BSW degree from a politically stimulating but intellectually banal program in Social Work at Bombay University. The *paan* chewing head of the Anthropology Department at Pune University cagily advised me, "Why come here? Go to Deccan College. Do archaeology. Anthropology here is mediocre. At least in Deccan College you will be in the best Institute in Asia." He further elaborated, casually but pragmatically justifying his logic that was difficult for me to grasp: "What difference does it make if you study dead civilizations or living cultures? Humans made them both. It is the same thing. Go right now. I will call the Director right away, he is a good friend and he will admit you." And that's how this journey that theoretically oscillated between the disciplinary boundaries of archaeology and anthropology began, accruing a debt of innumerable teachers, colleagues, friends, and people who have touched my life for more than a decade.

Although this is an unlikely location, but I would still like to take the opportunity to show my appreciation to my professors and colleagues at Deccan College (from 1994-96) who instilled a belief in me the possibility of studying past as a conceptual entity. Especially, I would like to thank Prof. K. Paddaya for opening up the intellectually simulating world of theoretical archaeology; Vasant Shinde for making archaeological excavation exciting; Ajay Dandekar for extraordinary support and mentorship. I am thankful to the friendship and companionship of Rajan Chedambath, V. Slevakumar, Shriram Joshi, for making those two years in the DC hostel memorable along with Kaushik Gangopadhya, Abhijit Dandekar, Abhinav Goswami, and Syam Panda.

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My career at Stanford has been linked to the genesis of two academic experiments – the Cultural and Social Anthropology (CASA) department and the Stanford Archaeology Centre. I have had the enviable privilege to be part of both these intellectually invigorating communities, which have shaped my academic development. The buzzing excitement of the newly evolving Archaeology Centre, with its constant stream of visitors from the world of archaeology, eclectic workshops, intense student conferences, and a slew of course offerings has had a great impact on my academic life at Stanford. I would like to thank John Rick and Ian Morris for providing me the opportunity to work at the sites of Chavin du Huantar, Peru and Monte Polizzo, Italy respectively. The erstwhile CASA with its close-knit community and theoretically open environment has played an important role in shaping up my anthropological practice. I would like to thank Renato Rosaldo, Paulla Ebron, Lynn Meskell, and Miyako Inoue for taking interest in my work. I would also like to thank Neepa Acharya for taking time out from her schedule for meticulously reading multiple drafts of this dissertation.

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Contents

Chapter 1

Notes on Postcolonial Archaeology

On Marginal Modernity

This dissertation is about two marginalized configurations of modernity – archaeology and the postcolonial state – the former a disciplinarian formation and the latter a system of governmentality. Marginalized because both archaeology and the postcolonial state represent fractured symbols of authority and power within the imagination of modernity. Their objective valence is peripheral in the domain of knowledge production and governance – they have marginal influence in the domains they are part of – science and state respectively. By marginality, I mean conditions that are characterized by isolation, in-between-ness, and ineffectuality[1]. Within the hierarchy of science, archaeology has a troubled epistemological claim to objectivity and has been often consigned to the hybrid zone between the Sciences and the Humanities – tethering epistemologically between both the disciplinarian universes. On the other hand, the postcolonial state is a failed project. Unable to shrug off the shackles of colonial baggage, it has struggled to emerge as a capable instrument of governmentality. In this dissertation, I interrogate the location of their union, a site where they impact each other's performative practices principally in terms of the epistemic valence their power generates – the postcolonial archaeological site.

Science in the nonwestern world, for instance, postcolonial science, has been historically comprehended as a trajectory following the advances in the west, which pushes it into the periphery of scientific research initiatives (Chambers & Gilleepie 2000; Anderson 2002). Science studies has been dominated by an obsession with 'big' science, 'real' science, and 'techno' science – high-energy physics, biosciences, new media, and other dominant scientific enterprises. These disciplines provide both high visibility and epistemic vulnerability, making them useful while investigating the sociological and historical workings of science – its contradictions, incongruity, and its epistemic power. Non-big sciences as well as scientific practices in the non-western world have often been relegated to the scholarly margins of science studies. Disciplines like archaeology have been consigned to the fringes of sociological, historical, and philosophical deliberation within science studies. On the other

[1] Here I am very specifically differentiating between Appadurai's 'alternative modernities', Lisa Rofel's 'other modernities', Marilyn Strathern 'new modernities' or Marshal Sahlins's 'indigenous modernities' (Appadurai 1991; Strathern 1999; Rofel 1999; Sahlins 1999 respectively).

hand, the history of archaeology and archaeological practices has consistently focused on the disciplinarian trajectory in the Euro-American world, in the process, relegating trajectories of non-western archaeology into the periphery of disciplinary scholarship. The History of state, governance, and governmentality has also emphasized the ideological trajectory in Europe and has only recently attempted to comprehend the nature of the postcolonial polity and its statist ideology. However, the postcolonial state is often viewed as a failed project of modernity (Carment 2003; Clapham 1998; Jackson 2000; Zartman 1995; Sinha 2005; Hill 2005) and at best continuing vestiges of the colonial governmentality (Pels 1997; Kaviraj 1997; Chatterjee 1994, 2004; Kholi 1990). In this dissertation, I attempt to engage with both these marginal forms of modernity – archaeology and the postcolonial state – in order to examine their ideological and conceptual configuration and its articulation. I do this by studying the processes of practice of one formation – archaeology, a marginal science – subsumed within the ideological grasp of another marginal entity – the postcolonial state.

This dissertation attempts to study both these marginal expressions of modernity in a non-western setting – postcolonial India – with two theoretical goals: to *provincialize modernity,* and to comprehend the articulation of this *provincialization* process (Chakrabarty 2000). Although the project of provincializing modernity has primarily been undertaken as a historical initiative, in this dissertation, I deconstruct provincialized modernity as an ethnographic project. The core question that this dissertation attempts to answer is: how is modern science provincialized within the ideological machination of the postcolony? Specifically, I investigate the disciplinarian framework of archaeology as a scientific practice carried out by a nationalist, statist and bureaucratic postcolonial state. My ethnographic intervention focuses on the daily processes of archaeology as an epistemological project with a deep-rooted colonial genealogy, articulated as a scientific practice in postcolonial India under a bureaucratic institution during an ultra-nationalist period. It is an account of the social and epistemological intervention of the Archaeological Survey of India (ASI) – one of the oldest and largest statist archaeological organizations in the world – located at the fringes of the postcolonial state. So, in effect, this dissertation is anthropology of marginality – a marginal science articulated by a marginal state at the margins of its boundaries, theoretically envisaged as a post-Kuhnian, postcolonial investigation of archaeology as a science.

The ethnographic description of this dissertation centers on the sociological and the epistemological working at an emblematic site – the archaeological excavation. Emblematic because, for archaeology, this is the most significant epistemological location from which the discipline derives its foundational objective and scientific authority. It is a location comparable to the laboratory of 'real' sciences – a site symbolizing the empirical universe of archaeology – here, evidence is discovered, knowledge is created, and objectivity is performed. In the process of systematically unearthing accumulated past cultural remains, archaeologists generate empirical evidence for the construction of past narratives. The metaphor invoked by Peter Galison, when talking about the goals of the experimentalist in high energy physics, I think also aptly applies to the relation between archaeologists and their excavations, "they are like the relationship of Michelangelo's *David* to the block of marble from which it was hewn: the statue is in stone, but the background has to be carved away in order to see it" (quoted in Lenoir 1997: 38). Through archaeological excavation, a landscape is carved out to produce a highly visual spectacle of past – scientific, performative, and political in its display. The earth is divested of the unwanted and what remains is a spectacular *epistemic spatiality*. I focus my ethnographic intervention on this location in which, through sociological and epistemological microprocesses, the archaeological material artifact is transformed into an *epistemic thing* (Rheinberger 1997). My research raises the basic question of how an artifact comes into existence and how it is shaped in the archaeological project. Through ethnographic investigation at the excavation site, I study how these epistemic things turn into the Foucaldian 'object-discourse' of the knowledge production mechanism of the postcolonial archaeological project. However the excavation site in this account is not just another location of epistemic production – it is a political charged, diametrically hierarchical, bureaucratic embodiment of the postcolonial state. Here the archaeological knowledge produced is simultaneously a product of the philosophical trajectory of archaeological epistemology and the political genealogy of the postcolonial bureaucratic formation.

Thus the knowledge produced at the excavation sites is an epistemic instantiation of power/knowledge - a product of the discursive domains of archaeology as a scientific discipline and of the administrative regime of a bureaucratic system. Through ethnographic inquiry, I uncover the embodied/enacted everyday practice of this spatialized discursive location. I specifically focus on the complexity of its knowledge production processes to comprehend the provincialized performance of marginal modernity. The study of the margins

and the peripheral is politically essential to gain insight into the epistemological and structural workings of the center (Das & Poole 2004), in this case, the processes through which the modern state creates scientific knowledge. I argue in this dissertation that it is inherently the epistemic logic of archaeology and the social and cultural practice of its science that make it vulnerable to statist appropriation. I demonstrate that scientific archaeology is itself an ideological practice; the boundaries between construction of scientific evidence and its statist manipulation are non-existent and, in fact, present a post-facto perspective on the process of archaeological knowledge production.

Ethnography of Archaeology as a Science

The post war trajectory of archaeology as a scholarly practice has involved a consistent struggle with its own disciplinarian subjectivity (Flannery 1967; Clark 1973; Hodder 1991; Preucel 1995; Van Pool & Van Pool 1999; Joffe 2003). The rise of science and scientific imagination having powerful cultural, social and political authority specifically after its resurgence in the atomic age saw social sciences scourging for its own scientific legitimacy (Barns 1974). Historically by the end of the nineteenth century, archaeology, like most social sciences, viewed itself as a science in terms of the empirical knowledge that it created (Daniel 1950, 1975; Trigger 1989; Christenson 1989; Kehoe 1998; Schnapp 1993). It was only in the early 1960s that a concerted attempt was made to refurbish its objective authority - archaeology was then determined to be closer to the subjective practices of historical and cultural approaches. Influenced by anthropology's assertion to objective claim to knowledge (Gordon & Phillips 1958, White 1959; Trigger 1998; Lyman 2007), archaeology, in the garb of 'processual archaeology', saw itself shedding its culture-history model of knowledge construction for a robust analytical emphasis (Binford 1962, 1965, 1972; Binford & Binford 1968; Clark 1968; Watson, LeBlanc &. Redman 1971; Salmon 1978; Dunnell 1981; Schiffer 1987, 1988). The culture-history model, originating in the late nineteenth century, had gained its scientific legitimacy by employing the received wisdom of geological sciences, the systematic process of excavation, and the usage of typological and classificatory analytical frameworks (Dunnell 1978; Lyman O'Brien & Dunnell 1997; Lyman & O'Brien 2003). On the other hand, its successor – processual archaeology – predisposed by cultural-evolution theories of predictable changes, attempted to reinforce archaeology's objective claim to the past by the rigorous application of scientific method – creation of data through observation, experimentation, and employment of hypothetico-deductive reasoning. This gave rise to the

environmental deterministic view of past, lacking in human agency, and de-legitimizing cultural categories accompanied by an overt proclamation of objectivity based solely on rigorous application of scientific method. Christianized as 'New Archaeology,' it propelled an objective view of the past, and had a widespread methodological impact throughout the world of archaeology; however its 'scientific' ascendancy was short lived. The rise of post-structuralism delivered a severe critique of processual archaeology and its scientific practice – indicting its claims of objectivity as a positivist, determinist, and faulting its narrow theoretical apparatus for being incapable of comprehending the fluidity and the flux of past cultural systems (Hodder 1982a, 1982b, 1986, 1989, 1992; Shanks & Tilley 1987a, 1987b; Tilley 1994; Bapty & Yates 1990). This frontal assault on the scientific assertions of processual archaeology by proponents of post-processual archaeology led to acrimonious debates about the theory and practice of archaeology, in the process, aggravating the crisis of its own disciplinarian subjectivity struggling between the earlier debate between history and science (Hodder 1984; Hodder et al 1995; Shanks 1992).

This internal struggle was made more acute by the widely heterogeneous discursive genealogy of archaeology within the Euro-American world. In America, archaeology is disciplinarily aligned to anthropology (Willey & Sabloff 1993; Reyman 1992; Kehoe 1998;), whereas in Europe, it is closely associated with history (Tigger 1989; Hodder 1991). Furthermore, the tension in the twentieth century trajectory of archaeology between science and nationalism also brought archaeology into the forefront of the politics of the modern nation-state (Arnold 1990, 1992; Kohl & Fawcett 1995; Diazi-Andreu & Champoin, 1996; Meskell, 1998;). The appropriation of archaeology in constructing nationalist ideologies by European nations presents a vivid illustration of this deep-seated tension (Trigger 1984, 1989; Ucko 1995; Jones 1997). After World War II, this link between archaeology and nationalism has been central to the making of newly decolonized nations like India (Kuklick 1991; Abu El-Haj 2001; Shepard 2002). By the early 1990s, archaeology was in the throes of a serious crisis of its disciplinary subjective – coming to terms not just with its heterogeneity in theory and practice but also the regional disjuncture of the methodological framework that it had considered homogenous. These factors were the subtext underlying the homogenous assertion of the World Archaeological Conference (WAC) – it was the most explicit organizational symptom of the disciplinary crisis in archaeology (Ucko 1990; Shanks & McGuire 1996; Zimmerman 2006).

Specifically in India, this crisis of the disciplinary subjectivity of archaeology (Paddayya 1990) was very powerfully embroiled in the nationalist subversion of archaeology. The political blending of science, history, nationalism, and mythic past was an intrinsic historical process through which archaeology evolved in postcolonial India – culminating in the demolition of the Babri Masjid in 1992 (Berbeck & Pollock 1996; Shaw 2000). For all those of us who were involved in the archaeological and historical knowledge production of the Indian past, this disciplinarian and ultra-nationalist collusion was apparent (Engineer 1992; Mandal 1993); however it came to the attention of the international archaeological community in a disturbing way during the 1994 WAC in New Delhi (Murlidharan 1994; Rao 1994, 1999; Hasan 1995; Colley 1995; Golson 1996). The destruction of the sixteenth century Babri Masjid in Ayodhya by Hindu fundamentalists brought into sharp focus the politics of ethics (Vitelli 1996), science of archaeology, state and nationalism not just in India but also in the world of archaeology at large. The community was faced with issues like destruction and deterioration of heritage monuments (Meskell 1998, 2002; Lowenthal 1998); the commercialization of archaeological practice; repatriation of antiquities (Mihesuah 2000; Thomas 2000; Tamara 2001); international trade in illegal antiquities (Brodie & Renfrew 2001); assertion of the rights of indigenous people (Layton 1989; Atalay 2006) and the excessive subversion of archaeology by politics (Gillian 1994; Hamilakis 1996; Jones 1997; Lamberg-Karlovsky 1997; Gathercole & Lowenthal 2003). These issues, together with predicaments about disciplinary subjectivity have resulted in archaeology's need for re-assessing historical antecedent of its theory and practice – a continuous process of which this dissertation is also a part.

During the last few decades of scholarship, attempts to disentangle the various tensions in the disciplinary subjectivity of archaeology have consistently been made in the meta-theoretical realm (Fleming & Johnson 1990). A fixation that was born with excessive theory building in the discipline during the rise of processual archaeology (Binford 1962, 1972; Clarke 1968; Schiffer 1976, 1987) and prospered during the bitter struggle with post-processual archaeology – a symptom of its own disciplinary insecurities – which continues to frame contemporary debates and discussions. This meta-theoretical approach gave rise to the need to write a history of archaeology, concentrating on the discipline as a cultural and political practice which evolved as a scientific discourse closely linked to the ascendancy of the nation state in Europe and the expansion of colonialism (Trigger, 1984; 1989; Ucko, 1995;

Robertshaw 1990; Jones, Graves-Brown & Gamble 1995). This effort at theorizing the praxis of archaeology within a larger meta-historical shift often ignored the historical genealogy of its disciplinarian trajectory in the context of the ideological genesis of its methodologies. In India, this was followed by the writing of a sub-continental history of archaeology, locating the evolution of the discipline in the colonial context (Chakrabarti, 1988, 1997; Padayya 1995). Both these global and local historical projects were essentially undertaking the documentation of the socio-political genealogies of the discipline. These projects were extensive chronological accounts delineating the trajectory of archaeology in relation to larger meta-narratives of nationalism, colonialism, and imperialism. However, with recent post processual approaches, the need to study the micro-practices and micro-processes of archaeological fieldwork emerged, which gave rise to a history of the methodological practices of archaeology.

In the past decade, there has been a coherent attempt at going beyond meta-theoretical critique to explore archaeology's own archives to uncover the historical genealogy of the disciplinarian impulses and to delve at the edge of the trowel to interrogate the methodological basis of its practice– a project that theoretically frames my work. In these attempts, the focus was on studying the genealogy of fieldwork processes of archaeology rather than creating meta-narratives of disciplinary archaeology (Fotiadis 1993; Castaneda 1996; Andrew, Barett & Lewis 2000; Hodder 1999; Lucas 2001). Almost concurrently, the feminist critique of archaeological fieldwork theoretically attempted to employ ethnographic intervention to provide a critique of ethnographic methods (Gero 1994, 1996; Conkey & Tringham 1996; Politis 2001). Consequently, post-processual archaeologists in their pursuit of reflexive knowledge production (Hodder 2000; Asa 2001, 2003; Hodder & Asa 2003), introduced the ethnographer as an important actor in understanding the impact of archaeological intervention on the local community and to the study the nature of their own archaeological practice. This dissertation is a continuation of these initiatives of ethnographic intervention of archaeological practices (Edgeworth 2003, 2006; Bradely 2003; Holtorf 2002; Yarrow 2003). Like these initiatives, it radically shifts the site of inquiry from meta-narratives of archaeological theory to the actual stage where material culture is discovered, analyzed, and shaped into empirical evidence. Such a perspectival shift, I contend, can demonstrate the complex ways in which ostensibly scientific methods merge with nationalist ideologist and the bureaucratic practice of the state. In contrast to previous scholarship, I argue that science and ideology collapse in the

practice of archaeology and it is not merely the manipulation of data by ideological agendas that gives rise to nationalist archaeology.

My ethnographic intervention in the archaeological site is aimed towards an understanding of the scientific construction of knowledge as a social process interrogated by the discursive framework of sociology of science. Indebted to the works of Robert Merton, influenced by the paradigmatic *The Structure of Scientific Revolutions* by Thomas Kuhn (Kuhn 1965), and propelled into the forefront of science studies by the Edinburgh Schools' Strong Programme (Bloor 1976), sociology of science has emerged as an important disciplinary practice to understand the social structure of scientific knowledge production. Scholars working in these traditions are concerned with apprehending how scientific knowledge is produced in the laboratory, disciplinary, and broad social contexts, focusing on the method of scientific argumentation and negotiation. This inquiry has sought to demonstrate the inseparability of the social location from the manufacture of scientific knowledge, and has established that scientific knowledge is constructed, maintained, determined, and shaped by cultural practices (Pickering 1992; Haraway 1989, 1991; Latour 1987; Fuller 1993, 1997). These communities of scientists have been called 'epistemic cultures', analogous to the notions of 'knowledge societies' or 'information societies', which create knowledge through social, discursive, and material practices (Knorr-Cetina 1999). These are analyzed as heterogeneous practices of knowledge production conducted in the *laboratory* or the *field*, mediated by technological elements (instruments, equipment, tools), social factors (interests, goals, structures), and conceptual frameworks (representations, models, theories), that are irreducibly mutually constitutive (Knorr-Cetina 1981, Latour 1987, Latour & Woolgar 1986).

Alongside, this dissertation is also theoretically informed by the critique of science leveled by feminist critics, which has established that scientific discourse, embedded in the larger Western epistemological system is not only androcentric, Eurocentric, and not culturally transcendental, but is also bound by specific cultural histories and relations, resulting in a need for a deconstruction of its claims to objectivity (Haraway 1989, Harding 1986, 1991, 1998). Similarly, works which historically locate the scientific genealogy of other field disciplines like geology (Rudwick 1985; Laudan 1987; Secord 1986), paleontology (Rudwick 1972) and geography (Edney 1999) also provide the critical framework for discussion in this dissertation. These works have located the emergence of the disciplinarian formation of field sciences

within the discourse of Enlightenment, which according to them, strengthened the latter in the ideological context of colonial expansion. However, the main setback with this scholarly tradition, in my view, is that it has been unsuccessful in addressing the issue of knowledge production in a non-Western environment. In this dissertation, I employ these theoretical insights to investigate the peculiarities of knowledge production in a non-Western space - especially how science functions to legitimize a postcolonial nationalist project. It locates archaeology in this rise of science by engaging with the growing literature on the development of science as a colonial knowledge production system (Kumar 1995; Baber 1996; Arnold 2000, Prakash 1999) and its relation to postcolonial nationalism (Viswanathan 1985, 1997; Alvares 1992; Nandy 1988, 1995; Abraham 1998, 2000).

In this dissertation I present the ASI archaeologists working in the archaeological sites as members of a very specific "epistemic community" – scientists producing knowledge in a bureaucratic institutional set up. They are distinct from archaeologists working in university settings in India because of the significant statist authority they wield. Their domination in the area of archaeological knowledge production in India has legal legitimacy and objective sanctity because they are part of the statist bureaucratic machinery. The archaeologists of the ASI are simultaneously postcolonial bureaucrats and archaeological scientists – and as I show throughout this dissertation – there is a *disjunctural tension* between these two professional practices embodied in a single institutional organization that affects the way knowledge is produced at the archaeological site. The specific aspect of this epistemic community relevant to my arguments in this dissertation is that ASI archaeologists exemplify the disciplinary personification of marginal modernity – they are at once the symbols of the postcolonial state in the fringes of the nation and are simultaneously practitioners of a marginal science struggling to produce objective knowledge about the past. It is this dual character of this epistemic community that makes them distinctive from an organization that solely produces knowledge, because the ASI is significantly also an instrument of postcolonial governmentality.

Anthropology State and Governmentality

Since Edward Said's *Orientalism*, postcolonial theory has emerged primarily as a critical theory that uses multiple forms of representations – literary and historical texts – as the means to theorize the immediate impact of colonialism as well as its later repercussions (for instance

see Loomba 1998; Chids & William 1997; Chrisman & Perry 2000; Gandhi 1998; Mongia 1996; Moore-Gilbert 1997). The 'post' in postcolonial was intended to signify the temporal location of the theorists pontificating the preponderance of ideologies of the metropole in the colony. The postcolonial theorists rarely employed ethnographic archives to theorize about the colony. And for good reason; the emergence of postcolonial theory also saw a rigorous post structuralist critique of the objective status of the ethnographic encounter. Ethnography was discredited, because, in its contemporary form, it was viewed as the academic personification and continuation of the characteristic colonial encounter with the 'other' (Fabian 1983; Clifford & Marcus 1986). Thus, in postcolonial theory, a distinct disjuncture between representational text and ethnographic representations occurred in the archives that these theorists exploited. Postcolonial theory largely remained a theory about the colony rather than the postcolony as a result of poststructuralist condescension towards ethnography. In India, the emergence of the Subaltern school of historians provides a redoubtable example of such a text-centric re-examination of the colonial encounter – creating a conceptual antidote to the metropole-consumed theory of the colonial encounter (Prakash 1992; 1994; Guha 1997; Guha & Spivak 1988; Chaturvedi 2000; Chakrabarty 2002). However the formulations of the postcolony were largely ignored, because the postcolony was dominated by the realm of the contemporary – the ethnographic, and also the postcolony was thought to have been explained by the socioeconomic theoretical framework of developmental studies, globalization studies and its most recent incarnation – studies of neo-liberal formations. Thus, the postcolony was largely absent from postcolonial scholarship. However, it is through the anthropology of state and governmentality that postcolonial theorists are now attempting to shift their gaze from the colonial encounter into the contemporary reality of the postcolony (Gupta 1998; Agarwal 2005; Moore 2004; Mbembe 2000).

Until recently, the state was not an object of study for the anthropologist; it was relegated to the disciplinarian interrogation of political science, sociology, and often economics. The beginnings of the anthropological interrogation of the state can be traced to numerous theorists that have inspired the trajectory of anthropology - from Weber, to the Marxist Gramsci, and post-Marxist Althusser. However it is with the rise of Michel Foucault inspired governmentality studies (Foucault 1991) that the state as an object of anthropological inquiry made an analytical resurgence like in other disciplines (Fergusson & Gupta 2002; Rose & Miller 1992; Barry *et al*; Baruan 2000; Bratich *et al.* 2003; Agarwal 2005; Walters and Haahr

2005). Foucault's examination of the ideological ramifications of modernity or "bio-power" was distinctly formulated between two distinct analytical poles – "'the anatamo-politics of the human body,' the anchor point and target of disciplinary technologies, on the one hand, and a regulatory pole centered on population with a panoply of strategies concentrating on knowledge, control and welfare" (Rabinow 1996: 91). The first concerned the excretion of disciplinary power over the private body as a coherent political technology to produce a bourgeois citizen – a docile subject of capitalist society – regulated in church, prisons, schools and hospitals. The second was governmentality, through which Foucault explained how the sovereign state shifted its focus on territorial jurisdiction to a control characterized by the effective category of population. He conceptualizes the state as an operative network ensemble through which it articulates the "analytics of government" (Dean 1999). In order to comprehend the working of such an operative system, Foucault argues for the adoption of a theoretical approach in which power is fragmented into political rationalities, technologies and techniques of governance. The analytical focus is on the practices of regulation in micro-settings with an emphasis on investigating the technologies and assemblages of governance, and interrogating the operations of materials, agents, and techniques by the state to put these governmental rationalities into practice. The aim was to examine the particularistic apparatus of the government in all its specificity rather than providing a general theory of the state. The impetus was to comprehend the "how" of government (Dean, 1999).

This emergence of governmentality studies has brought in a new level of critical investigation in the anthropology of state (Fergusson & Gupta 2002; Agarwal 2005: Sharma & Gupta 2006; Scott 1998; Taussig 1992; Mitchell 1991, 2002; Hansen & Stepputat 2001; Mbembe 1992; Das & Poole 2004; Fuller & Benei 2000). By the early 1990s, anthropologists recognized the consequences of statist intervention in the material and the subjective formation of the 'local' – the normative center of anthropological examination. Untill recently, anthropologists paid little attention to the cultural dynamics of the modern state; however the impact of state and bureaucratic intervention on the communities has turned the state into an important operative to be studied. The new analytical approach to the state is informed by the view that the state is a powerful site of symbolic and cultural productions and not just the source of a bureaucratic apparatus for governance (Steinmetz 1999). It is not only the nation that is an imagined entity but also the state, which is made up of assembled ideological formations that are conceptualized and made bureaucratically effectual through culturally potent symbolic

systems. Methodologically, this anthropology of the state calls for an approach that does not isolate the various functionalist personifications of the state, but argues for an investigation of the modern state as a combination of its various embodiments - political economy, institutional apparatus, social structure, everyday practice, representation system, and its ideological formations (Althusser 1971; Abrams 1988; Alexander 1997; Bourdieu 1994; Bayart 1997).

In this dissertation, I investigate the ASI as an emblematic organization imbricated in the institutional practices of the postcolonial state. The ASI is a bureaucratic organization which is not only involved in the organization, protection, and management of archaeological and heritage sites in India but is also significantly involved with the construction of knowledge about the past. It is this combination of bureaucratic work and scientific knowledge production institutional apparatus that makes it a productive site for investigating the nature of science and state (Mukherjee 1989; Carroll 2006). I examine the everyday practices of the ASI bureaucracy at the archaeological site to study the representations and micro-markers created by its institutional and discursive agency.

The choice of the ASI to conduct this ethnographic work emerges from a number of reasons: The ASI was not just one of the oldest archaeological organizations in the world but it was also one of the largest of its kind, with enormous control over the archaeological heritage in India. Unlike other parts of the world, Indian laws did not allow private cultural resource management organizations to conduct archaeological excavations in India, thus making thr ASI a very powerful organization in comparison to the handful of University departments and small provincial archaeological organizations producing archaeological knowledge. By a series of legislations dating to the late nineteenth century, the ASI was given the custodianship of archaeological heritage in India. It was not possible to excavate any site in India without a license issued by the Director General of the ASI. Over the course of a century and half, the ASI had created a huge archive of knowledge about the Indian past and it far surpassed any other archaeological organization in South Asia. It also had jurisdiction over a huge collection of artifacts and material culture, which had been discovered and excavated since its inception (other than those that were shipped to England during the colonial era). Thus the ASI had hegemonic authority on the archaeological heritage of India; no other statist or non-statist actors could overrule its power. In these circumstances, the ASI becomes a unique site to

study the intersection of archaeology, science, and governmentality.

Archaeological Survey of India - 1861-1947

The ASI has the distinction of being one of the earliest archaeological organizations in the world. It was established in 1861, soon after the Revolt of 1857 when the East India Company was relieved of their control over India and the colony came under the direct control of the British crown. This caused a considerable administrative shift in the way the colony was governed. From a policy of political occupation of territory and annexation of princely states, the emphasis shifted to extracting the surplus through governance and administrative means (Bayley 1988; Guha 1997; Chatterjee 1993). Although the history of the ASI as a colonial knowledge production agency was not located directly in such a shift in governance, it was undoubtedly a cause for its establishment – a responsibility of "an enlightened ruling power" (Lord Canning, cited in Chakrabarti 1988: 57). However its ideological genealogy was situated in two distinct forms of colonial engagement with the 'other', framed by the necessity to accumulate knowledge about the colonized – historical and travel writings (Pratt 1992; Raman 2001; Rubiés 2002;) and the geographical mapping of physical territory for military exploits (Edney 1990). The establishment of the Survey of India in 1767 as an unambiguous military reconnaissance organization, and the creation of the Asiatic Society of Bengal in 1784 with the aim of studying Indian past, represented respectively the cartographical and literary 'framing' of the colonial India. The Great Trigonometrical Survey of India represented a military project envisaged to bring the entire subcontinent into the epistemological grasp of a cartographical framework (Edney 1990), whereas William Jones' identification of the linguistic resemblance between Sanskrit, ancient Iranian, and European languages laid the foundation for inquiry into ancient India.[2] Both these incipient cartographical and textual framings of the colonial universe were instrumental in the rise of the Indological project, and subsequently, the birth of the ASI (see Chakrabarti 1988, 1997; Paddayya 1995; Cohn 1996; Dirks 1994; Guha-Thakurta 2005).

The ASI was one of the several colonial organizations that provided information for the colony's adequate governance by rigorously producing *scientific* facts about India- its people,

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[2] On the impact of William Jones on the history of Indology see: Chakrabarti 1988; 1997; Paddyya, 1995; Trautman, 1997; Murray, 1998; Mukerjee, 1968; Kejriwal 1988.

its history, and its geography.[3] Colonial ASI's epistemological genesis was situated between what Bernard Cohn calls the 'historical modality' and the 'survey modality' (Cohn, 1996:5). Cohn lists these categories as some of the numerous 'investigative modalities' of knowledge production mechanisms which were invented by Imperial ideology and later perfected in colonies in order to produce facts that could be categorized and classified in order to control and govern their subjects Other such modalities include the 'observational/travel modality,' 'enumerative modality,' 'museological modality,' and 'surveillance modality' (Cohn, 1996). For Cohn, the 'historical modality' is a means of knowledge production, instrumental in 'the ideological construction of Indian civilizations' whereas the 'survey modality' is involved in 'mapping and bounding to describe and classify the territory's zoology, geology, botany, ethnography, economic products, history and sociology' (Cohn, 1996:7). The ASI emerged out of the combination of all of these modalities. On the one hand, it was an instrument of survey that discovered, excavated, and classified India's past, and on the other hand, it was an agency that provided concrete evidence for the construction of an ideologically loaded history of India's past through the analysis of architectural remains, epigraphical inscriptions, and archaeological excavations. In its postcolonial incarnation, the ASI continues to embody this colonial ideological and epistemological framework. The ASI employs this structural framing in the production of a nationalist construction of Indian civilization – a project similarly ideological in motivation and content.

The early years of the ASI in the second half of the nineteenth century were singularly marked by the archaeological exploits of Sir Alexander Cunningham who was appointed as the ASI's first Director General (DG) by the Viceroy Lord Canning in 1871. Employing observations written by seventh century Chinese travelers, Cunningham combined this archival material with the "topographical approach" (Paddayya 1995: 130; Chakrabarti 1988: 115) of a military cartographer. Traversing the subcontinent, his work brought a vast amount of area, hitherto archaeologically unknown, under the purview of the Indological archaeology (Imam 1966). He primarily focused his energy on the discovery of Buddhist monuments, which remained a scholarly fixation for most field archaeologists even after his retirement. Subsequent successors to the post of the DG's were so ineffective that the post of DG was abolished

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[3] Survey of India established in 1767; Geological Survey of India established in 1851; Botanical Survey of India established in 1890, Anthropological Survey of India established in 1945; Zoological Survey of India established in 1969; Forest Survey of India established in 1981 –to name some of the survey organizations that are still part of the government in postcolonial India.

(Paddaya 1995; 133). It was only in 1901 after Viceroy Lord Curzon appointed Sir John Marshall, that archaeological research recommenced. The intervening years had been a "bleak period" (Roy 1961: 73), dominated by philological concerns rather than archaeological.[4] Marshall organizationally reinvigorated the ASI and explicitly emphasized the need to not just excavate and explore new territories but also initiated a concerted policy of conservation based upon the findings of his influential *Conservation Manual*, which is still in use today (Marshall 1923).

Along with the discovery and excavation of Buddhist sites in central and eastern India, it is the association with the Harappan sites that colonial ASI is symbiotically linked to (Tarutman & Sinopoli 2002).[5] The discovery of the Harappan civilization in 1920s marked a paradigm shift in the practice and imagination of Indian past. No longer was the civilizational history of Indian antiquity considered to materially originate with the Mauryan Empire (c. 322-185 BCE) or the subsequent spread of Buddhism. Neither did it temporally correspond to the emergence of Aryans in India. The excavation of Harappa by Daya Ram Sahni, Assistant Superintendent of the ASI in 1921-22; and of Mohenjodaro in 1922-23 by Rakhaldas Banerji, Superintendent of the Western Circle, and Madho Sarup Vats, officiating Assistant Superintendent of the Western Circle in 1923-24, together with the public announcement of the discovery of a new civilization in the *Illustrated London News* in London by John Marshall, ushered a new era of archaeological imagination in India. Prior to this discovery, Indian archaeology had been fixed by Buddhist material culture, medieval Hindu temple architecture, epigraphy, and numismatics. Other than a few colonial surveyors who had collected lithic tools, prehistoric material culture did not have a pre-eminent place in colonial

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[4] J.B. Kieth a senior officer of the ASI who had headed the North Western Circle wrote to Lord Curzon in 1900, while the search for a new DG was on: "For years the necessities of Indian Archaeology have fallen into the hands of philologist who have made a preserve in pressing home the exploded theories of professor Max Muller, largely responsible for the exaggerations of Western civilization" (cited in Lahiri 2005: 45).

[5] It is important note the relationship with Buddhist monuments and Harappan sites in the scholarship of colonial archaeology. All the archaeologists and explorers who visited the site of Harappa and Mohenjodaro before these were officially declared sites of the 'Indus civilization', viewed them through the framework of Buddhist archaeology. From Alexander Cunningham to R.D Bhandarkar, none were able to comprehend their pre-Buddhist significance, because they were searching for Buddhist monuments. It was a series of chance discoveries and the "brilliance" of two colonial native archaeologists under the leadership of the then DG of the ASI that led to the discovery of the civilization (see Lahiri 2005 for an engaging account).

Indian archaeology. With the discovery of Harappan civilization, proto-historic archaeology came to the forefront in the search of Indian antiquity.[6] Organizationally, this discovery was the catalyst in the formation of the Exploration Branch with a Deputy Director General and three Assistant Superintendents. However, there was a shift only in the way that Indian past was imagined and administered, not significantly in the methodology of archaeology. It was the arrival of Sir Mortimer Wheeler that effectively marked a paradigmatic shift in the methodological practice of Indian archaeology.

Administratively, the ASI has been an evolving organization since its inception. Like most colonial organizations, it has been a sluggish bureaucratic institution, with inadequate funding and unimaginative superiors, lethargic to change and transformation for most of its colonial career. For example, it was in 1848 when Alexander Cunningham was a Second Lieutenant of the Bengal Engineers, that he formulated a proposal for an Indian Archaeological Survey. It was only in 1861 that he could begin work after being appointed as an Archaeological Surveyor. The ASI was saved from disintegration solely because of the interventions of some visionary British viceroys like Lord Canning, who was instrumental in the establishment of the ASI, and Lord Curzon in 1901. Not surprisingly, historians of the ASI summarily divided the whole colonial trajectory of the ASI into three periods based on the tenure of three DGs with creative leadership – Cunningham, Marshall, and Wheeler (see Chakrabarti 1988, Paddayya 1995; Lahiri 2005; Singh 2004). During the first fifty years of its existence, the ASI saw numerous financial and administrative crises – including a recommendation for the abolishment of the DG's post and a serious state of affairs in 1895 when the government of India requested the Asiatic Society to bear the ASI's responsibility. By 1899, administratively, the ASI was divided into five Circles - Bombay with Sind and Berar; Madras and Coorg; Panjab, Baluchistan and Ajmer; Northwestern Provinces with Oudh; Bengal and Assam. With John Marshall becoming the DG due to the intervention of Lord Curzon, the focus of the ASI shifted to conservation and protection of the archaeological heritage with the enactment of the Ancient Monument Preservation Act of 1904. In 1921, the ASI was declared a central governmental organization under the provision of the 1919 Montagu-Chelmsford Reforms

[6] Research in prehistoric Indian archaeology was pioneered by Philip Meadows Taylor who discovered Megalithic sites in the Deccan during his service with the Hyderabad state (1824-58) and Robert Bruce Foote of the Geological Survey of India who discovered close to 450 Paleolithic sites in Southern India and Gujarat (Taylor 1941; Foote 1866; 1914; 1916; also see Paddayya 1995; Chakrabarti 1979 for a historical account)

(Chakrabarti 188: 127), which led to the institution of a secure system of financial stability and a structured bureaucracy. However, by the1930s, administrative sloth had again set in and Leonard Woolley, the celebrated excavator of the 'Royal' cemetery of Ur, was appointed as a "foreign expert" to evaluate the state of the ASI. In 1939, Woolley submitted a controversial report, which was not published because of its critical and damaging analysis (Paddayya 1995: 134; Posshell 1993: 1). Among several recommendations, Woolley stated: "Outside help is necessary if any good is to come of the department's work" (Posshel 1993; 45). His comment was eventually responsible for the appointment of Mortimer Wheeler as the DG of the ASI in 1944.

Wheeler's role at the helm of the ASI in restructuring Indian archaeology, and the enormity of his contribution towards disciplining Indian archaeology in four years (1944-48) has been described as a series of developments that would have taken the erstwhile bureaucracy forty years (Paddaya 1995). The central objective of Wheeler's intervention was to rectify the ills of the ASI that were reported by Woolley. This goal had a disciplinarian impetus and was not only aimed at restructuring the institutional apparatus of the ASI, but also the epistemological efficacy of its knowledge production mechanism (Chadha 2002). The most crucial event of this transformation was the Taxila School of Archaeology in 1944, led by Wheeler. This was probably the first organized school of field archaeology in the history of the discipline. It was to play an influential role in the post-colonial trajectory of Indian archaeology, especially so, because students from this training camp emerged to head various archaeological departments throughout the country and run the ASI for the following few decades (Chakrabarti 1988:176). It was at Taxila and the various excavations that Wheeler conducted during these years at the sites of Arikamedu, Brahmagiri, and Harappa that he finally inscribed on the ASI ideas that are the focus of this dissertation: the notion of scientific excavation, the importance of stratigraphy, and other archaeological methods related to the production of archaeological knowledge. All commentators and historians of Indian archaeology have underscored Wheeler's influence on Indian archaeology in very clear terms (see for example: Clark 1979; Chakrabarti 1988; Paddayya 1995; Chadha 2002).

Wheeler's contribution to the institutional restructuring of the ASI was primarily administrative and but it is his contribution towards the theory and practice of Indian archaeology which has had a tremendous impact. An impact, as will be obvious, that echoes

throughout this ethnography conducted half a century after Wheeler's dominant tenure from 1944-48, as the DG of the ASI. Mortimer Wheeler is the *leitmotiv* of this dissertation. During my ethnography, I observed very clearly that his methodological shadow forms a heavy backdrop to all archaeological practices of contemporary ASI. Before I began this ethnography, I spent a considerable amount of time doing research on the impact of Wheeler's tenure on the colonial ASI (Chadha 2002). I was surprised to observe during my fieldwork that his influence heavily informed the methodological and theoretical universe of ASI archaeology. Not much has changed, I argue in this dissertation, since his intervention during the last years of the colony; and it will not be an exaggeration to state here that this dissertation is as much an ethnography of contemporary ASI as it is an ethnography of Wheelerian archaeology.

Postcolonial Trajectory of ASI

In 1947, the partition of India forced the newly postcolonial organization to reevaluate the archaeological heritage that came under its purview. Wheeler's tenure ended in 1948, and subsequently, he was appointed as an adviser to the Department of Archaeology in Pakistan. The ASI had relinquished jurisdiction of a substantial portion of the Old Frontier Circle, covering the entire region of the erstwhile West Pakistan, and parts of its Eastern Circle, comprising areas in East Pakistan (contemporary Bangladesh). This necessitated the reconfiguration of the boundaries and personnel and the creation of new areas of operations - thus the number of circles went up to nine from seven (Thakran 2000: 45). Some of the most prominent Buddhist archaeological sites such as Taxila and the northwestern Buddhist Gandhara complex went to Pakistan. Furthermore, the loss of Harappa and Mohenjodaro represented the biggest blow to the organizational subjectivity of postcolonial ASI, as these sites had constituted the professional essence of the ASI in the last decades of its colonial *avatara* (incarnation). B.B. Lal, in his high profile 2001 Review of the ASI, called this a "stunning blow to its cultural heritage" (Lal 2001: 26). Soon after, under the leadership of A. Ghosh and B.B. Lal, by the early 1950s, began a systematic exploration of the Western states of independent India.[7] Eventually, these excavations led to the large-scale excavation of the Harappan sites of Lothal (1955-63), Rangpur (1953-56) Kalibangan (1960-69) and Surkotada

[7]Ghosh explored the dry bed of the Ghaggar whereas Lal concentrated on the upper Ganga basin. Ghosh discovered 150 sites representing Harappan, Panited Grey Ware and successive cultures (Thakran 2000: 48).

(1971-72).[8] These sites can be seen as the epistemological precursors to the archaeological excavations on which this ethnography is based. With the promulgation Constitution of India coming into effect in 1950, 'archaeology' was made a concurrent subject under the Seventh Schedule of the Constitution[9]. The ASI was now the central authority that was involved in all aspects of archaeological exploration and excavation: maintenance, conservation, and preservation of protected monuments and archaeological sites and remains of national importance; chemical preservation of monuments and antiquarian remains; architectural survey of monuments; epigraphical and numismatic studies; setting up and running site museums; training students in Archaeology; bringing out archaeological publications; archaeological expeditions outside India; horticulture operations in and around ancient monuments and sites; implementation and regulation of The Ancient Monuments and Archaeological Sites and Remains Act, 1958 and The Antiquities and Art Treasures Act, 1972.

In 2004-05, the ASI was an organization attached to the Department of Culture, Ministry of Tourism and Culture with its headquarters in New Delhi. Being an attached office, the ASI has its own head designated as Director General who is assisted by an Additional Director General, a Joint Director General and a group of Directors. Administratively, the country is divided into 24 Circles, each headed by a Superintending Archaeologist (SA) responsible for the upkeep of the protected monuments in its jurisdiction. Alongside, there are six Excavation Branches, one Prehistory Branch, one Building Survey Project, two Temple Survey Projects, two Epigraphy Branches, and one Science Branch functioning in the ASI. Of these numerous wings in the ASI for conducting specialized archaeological research, the most prominent are the six Excavation Branches, that Wheeler first instituted during his tenure. However, any of the Circles, and various departments, including the Institute of Archaeology – the

[8] Sites of Banawali, Kuntasi, Daimambad, Rodji, Padri, Banawali, Bhagwanpura, Kunal, were some of the other prominent Harappan excavations till the 1980s.

[9] The composition of the Central Department of ASI in 1949 consisted of: (I) the supervisory staff, comprising Director General, Joint Director General, Deputy Director General for Administration and Deputy Director General for Explorations; (2) the technical staff consisting of Archaeological Engineer and Assistant Engineer, Archaeological Chemist and Assistant Archaeological Chemist at the headquarters; (3) the specialized staff comprising Epigraphical Superintendent, Assistant Superintendent and Assistant Superintendent for Arabic and Persian inscriptions in the Epigraphy Branch. Superintending and Assistant Superintending Archaeologists were appointed in the nine circles to organize and supervise the work in each circle. (Takran 2000: 47).

archaeological school of the ASI, can conduct archaeological excavations. By 2004-05, the ASI was a large postcolonial governmental organization, which employed a few thousand workers throughout the country, was responsible for the protection of 3663 monuments, and had excavated close to 292 sites since independence (Basu 2005: 5). Its annual budget in 2004-2005 was Rs. 223.30 crores, whereas for the year 2005-6 the total allocation was Rs. 251 crores, which was nearly 30% of the total budget of the Ministry of Culture.

The evolution of the postcolonial ASI has been marked by a series of review committees set up by the government to assess all the departments of the ASI every couple of decades. This evaluative convention that originated with the 1939 Woolley report, has continued since. Until 2001, there have been three similar 'high profile' reviews of postcolonial ASI, the committees including senior bureaucrats, ex DGs of the ASI, independent specialists, and university academicians. These review reports have been bureaucratic events through which those who had been involved with the ASI, leveled systemic criticism against the working of the ASI, and recommended significant changes. These reports have had an emblematic status in the career of the ASI as a statist institution and represent both its apathy to transformation and its lack of agency – arising from the maze of systemic entanglements of postcolonial bureaucracy. The review reports informed the organizational subjectivity of the ASI as a postcolonial bureaucracy as it evolved and transformed into a powerful statist organization with an annual financial outlay of considerable proportion.[10] The first of these was the Wheeler Review Committee Report of 1965, followed by the Mirdha Review Committee Report of 1984. The most recent was the B.B. Lal Review Committee Report of 2001, whose reverberations I felt throughout my fieldwork. These review committees reports had a quasi-legal status and were responsible for bringing about major bureaucratic changes, causing a significant change in the organizational transformation of the ASI. The recommendations of these review reports were taken seriously by the bureaucratic system, but its implementation was more often than not delayed by at least years, if not decades. Of the several recommendations that gathered dust in the files of the postcolonial bureaucratic system, one of them had the potentiality of transforming the professional essence of the ASI.

[10] In contrast to ASI's 2004-2005 budget of Rs. 223.30 crores, the total financial outlay for the Geological Survey of India was Rs. 176 crores in 2003-04; Anthropological Survey of India's budget was only Rs. 13 corers in 2004-05.

In 1984, the Mirdha Committee Report announced that, based on the context and content of the work that the ASI conducts, the organization should be declared a scientific organization: "The Archaeological Survey of India should not be considered merely an administrative organization; in view of its highly specialized functions, it should be accorded the status of a scientific institution, enjoying autonomy in its functioning, like other comparable institutions under the Government" (Basu 2005: 8). This mandate was the outcome of decades of the desire on part of the ASI archaeologists to be considered as scientists. The organization considered itself scientific on the basis of its disciplinary intervention with regard to protecting the heritage of ancient India and more importantly, its knowledge production. All the other major survey organizations came under the purview of the Department of Science and Technology, and there was a simmering professional discontent that the ASI was still attached to the Minister of Culture and not considered a scientific organization. However, the concern also had professional ramifications – the most prominent amongst these was the possibility of acquiring a higher pay scale. The issue was both a matter of professional status and disciplinary subjectivity. In 1989, a group set up by the Department of Science and Technology further recommended that the ASI be declared a Scientific and Technological Department; however the shift has not occurred to date. The 91st Report of the Department-related Parliamentary standing committee of transport, tourism, and culture, devoted to the functioning of the ASI, headed by the CPI(M), MP, Nilotpal Basu, and tabled in the Rajya Sabha on 25 November 2005, severely pronounced:

> The Committee is constrained to note that even after a lapse of fifteen years from the date of issue of the notification in this regard, no concrete action was taken by Ministry of Culture and Archaeological Survey of India for developing Archaeological Survey of India as a Scientific and Technical Department, which amply indicates the administrative apathy towards the whole issue...The Committee observes that there has been deviation in the working of Archaeological Survey of India and that it has failed in terms of developing Archaeological Survey of India, not merely as an administrative body, but also as a spearhead for consolidating the scientific discipline of archaeology in the country. The Committee is of the view that the Archaeological Survey of India needs to reinvent itself, not merely as an administrative wing of the Government, but as an agency for protecting and safeguarding our national heritage, which involves a lot of scientific and technical work. Unless the Archaeological Survey of India converts itself fully into a scientific

and technical organization, the basic role and function of the organization will be defeated (Basu 2005: 10).

The links between science, state, and bureaucracy within archaeology are best illustrated by the existing struggle between the Ministry of Culture, which does not want to transfer the ASI, its most financially prized organization under its ministry, and the Department of Science and Technology, which, although administratively willing to admit the ASI under its wing, was still disciplinary reluctant (Basu 2005: 9-10).[11] During the years that I was doing my ethnography, the ASI was struggling with its professional subjectivity. The tension was between its bureaucratic character as a large heritage management and conservation organization and its aspiration as a scientific organization. This tension – between governmentality and science; bureaucracy and archaeology – is at the heart of this dissertation.

In this dissertation, I specially focus on only one set of activities that the ASI has been conducting since its establishment –archaeological excavation. Archaeological excavation is a small part of the massive organizational work that the ASI is involved in, much of which is not dealt with in this dissertation – for instance conservation, preservation, protection of archaeological sites and monuments. Although financially, archaeological excavation is a small fraction of the total annual budget of the ASI, it is considered to be the most crucial task that makes up the organizational character of the ASI. Archaeological excavation is the key to the ASI's ambition of being considered a scientific organization – it is at the crux of its professional subjectivity as a statist organization involved in the production of objective knowledge. This dissertation is not about the overall working of the ASI, although I employ the ethnographic insights I have gathered during my ethnography of archaeological excavations in theoretically formulating my arguments about both postcolonial bureaucracy and archaeological science. The ASI archaeological excavation, for this dissertation, is a spatial, epistemological, and bureaucratic metaphor to comprehend the ideological workings of a postcolonial scientific bureaucracy.

As I have noted earlier, the ASI had conducted close to 292 excavations by the time I began my fieldwork, it included all types of archaeological sites in India – prehistoric, protohistoric,

[11] As the Basu Report noted: "The nature of functions and activities of Archaeological Survey of India were of such nature that it could not be so far made into a wholly scientific and technical institution. However, efforts have been made to take advantage of technical and scientific expertise in various disciplines of archaeology" (Basu 2005; 9).

Harappan, early historic, Buddhist, Jain, Hindu, Islamic and even British colonial sites. In the process, it had amassed a vast archive of archaeological knowledge about the subcontinent. It has exposed archaeological sites of monumental proportions, which have now become tourist sites. It had collected a huge amount of artifacts and material culture, some of which were housed in the various museums across the country and in the Central Antiquity Collection section of the ASI. It had published a large volume of reports, pamphlets, and books. This dissertation does not attempt to evaluate this massive knowledge production activity about Indian antiquity by the ASI. My ethnography is focused on a very particularistic epistemological activity that the ASI undertakes – the archaeological excavation of the Harappan civilization.

I chose Harappan Civilization as the epistemological location to conduct this study because the discovery of this pre-historic culture coincided with the rise of both scientific archaeology and nationalist discourse in colonial India (Chakrabarti 1988, Ramaswamy 2001; Lahiri 2005). The scientific rhetoric provided by archaeology gave this discovery an objective legitimization, whose profound impact continues to reverberate in the imagination of the postcolonial nation (Lal 1998, Gupta 1996, Ranatkar 2001). The consolidation of scientific archaeology, the rise of Indian nationalism, and the discovery of a new civilization have all had a significant impact on the imagination of an Indian past as articulated by the ASI. The discovery of an indigenous civilization, together with the emergence of nationalism during the independence movement, and the construction of a nationalist identity in the ensuing postcolonial decades, has played an important role in providing the Indian nation state with an objective past - scientifically proven and empirically tested. Particularly after independence, this process has been intimately associated with nationalism, where archaeological discourse merges with scientific practice to manufacture knowledge about ancient India. This intersection of scientific archaeology and ideology transformed the discourse on Harappan culture into a contested location for multiple theories and myths - nationalistic, imperialistic, postcolonial, and now, ultra-nationalist. My ethnography is specifically focused on the sites of one such intersection of state, science, archaeology and nationalism- the Saraswati Heritage Project (SHP).

Saraswati Heritage Project

Every year a large number of archaeological excavations are conducted in India. Along with the Excavation Branches and the Circles of the ASI, excavations are conducted by various state Directorates of Archaeology, university departments, and research organizations - Indian and foreign[12]. For instance in 2003-04, out of the total 173 proposals that were submitted to CABA for approval – projects ranging from large-scale excavations to exploration work, including 'trial trenching' and 'vertical digging,' 'scientific clearance[13],' 'trial digging,' photographic documentation - 54 belonged to the ASI[14]. Of these proposals, the most prominent was the Saraswati Heritage Project (SHP). The proposal submitted to CABA consisted of a plan to excavate fifteen sites by all the five active Excavation Branches of the ASI along with the excavation units of two Circles and a State Archaeology Directorate (see table. 1). The SHP was one of the largest multi-site excavation projects that the ASI had undertaken in recent years. The concerted plan of excavating multiple sites under one project by the ASI was last planned under the 'Archaeology of the Ramayana sites" in the 1970s, which had followed the "Archaeology of Mahabharata sites" in 1950-52. Both these projects had B.B. Lal at the helm of affairs and had had profound political consequences. They had been designed to authenticate the archaeological validity of the great Indian epic traditions. Fraught with controversies, the latter attempted to associate a ceramic type found in the Gangetic valley - the Painted Grey Ware (PWG) - with the Mahabharata, whereas the former was single-handedly responsible for providing an archaeological basis for the demolition of the Babri Masjid in 1992.

[12] According to the Ancient Monuments and Archaeological Sites and Remains (AMASR) Act of 1958, no excavation in India can be conducted without a license issued by the DG of the ASI. Technically any trained archaeologist in India with a PhD at the level of Reader in a University and at the rank of Dy.SA in the ASI or the State Directorate of Archaeology could apply to CABA with an excavation or exploration proposal. However the DG issued a license on the recommendation of the standing committee of CABA, after: "he is satisfied that having regard to the status of the applicant the competence of the director of excavation operations, the adequacy of the staff to be employed and other relevant factors, the license may be granted to the applicant" (AMASR 1958).

[13] 'Scientific clearance' was a term used in the ASI for small-scale salvage excavation or minor excavation conducted at heritage monuments necessary for conservation work.

[14] This also included proposal submitted by the Temple Survey Project, the Underwater Wing and the Building Survey Project of the ASI.

Excavating Agency	Sites
Exploration and Excavation Wing, DG Headquarter	Dholavira, district Kachchha, State Gujarat
Chandigarh Circle	Adi Badri & Kapal Mochan, district Yamunanagar, Haryana Thanesar & Sandhauli, district Kurukshetra, Haryana
Jaipur Circle	Kalibangan, district Ganganagar, Rajasthan
Exc. Br. I, Nagpur	Bhirdana, district Fatehabad, Haryana
Exc. Br. II, Delhi[15]	Pilibangan & Badopal, district Hanumangarh, Rajasthan
Exc. Br. III, Patna	Baror, district Ganganagar, Rajasthan
Exc. Br. IV, Bhubaneshwar	Chak 86 & Tarkhanwala Dhera, district Ganganagar, Rajasthan
Exc. Br. V, Vadodara	Juni Kuran, district Kachchha, Gujarat
Haryana State Archaeology	Jugni Khera & Bibipur Jheel district Kurukshetra, Haryana

Table. 1: SHP sites and the various excavating agencies.

Officially the SHP was initiated in 2002 by the ASI through a government of India gazette notification[16] leading to the constitution of a thirteen member committee called the 'Advisory Committee for the Multidisciplinary Study of River Saraswati,' under the chairmanship of the then Minister of Tourism and Culture. The project was aimed at, "conducting a multidisciplinary study of River Saraswati and its basin stretching in India from the Sivaliks to the Arabian Sea, falling in the Indian states of Haryana, Rajasthan, and Gujarat, and formulating and implementing integrated development programmes (sic) in the area by creating 15 hub sites as centers of culture, tourism, and good civic life" (Basu 2005: 11). Headed by the Jt. DG of the ASI, the focus of the project was a multi-disciplinary exploration of the river Saraswati, which not only included archaeological investigations but also consisted of geomorphological, geotechnological, hydrological, ethnological, palaeobotanical, palaeontological, pedological studies and a detailed analysis of historical literature and oral traditions. The objective of the proposal was:

> ...to define the River Saraswati and its tributaries in the basin by adopting a multidisciplinary approach: to identify special items of geotechnical studies; to

[15] By early 2004, I came to know that SA of the Exc. Br. II, Delhi had refused to excavate the site of Pilibangan as the site was disputed. It belonged to Muslim Wakf Board who since partition had been using the site as Muslim burial ground. Instead the Ex. Br. eventually excavated the site of Hasi, district Hissar, Haryana.

[16] S.O. 1329 (E), dated 18th December 2002, New Delhi.

promote multidisciplinary archaeological research by way of exploration, excavation and specialized studies such as ceramic, metallurgical, mineralogical, botanical, zoological, geotechnical, petrological, sedimentological; to carry out structural and chemical conservation of sites, monuments, and excavated structures as well as moveable objects; to accomplish environmental upgradation of the sites; realize all these objectives in an integrated, concerted and planned manner under a special drive during a period of three years, to begin with (Basu 2005: 11).

In September, the Advisory Committee prepared a project proposal of the SHP with a financial budget of Rs. 36.02 crores, which was eventually reduced to Rs. 4.98 crores for the period of three years (Basu 2005: 11). For excavation and exploration, the SHP was largely planning to harness the existing resources of ASI. Initially, the excavation was proposed at the prospective sites for a period of three years with a total budget outlay of Rs. 1.76 cores.[17] The academic thrust of the project was to conduct archaeological excavation of Harappan sites that had been discovered over the last few decades in the dry banks of river Ghaggar in Haryana and Rajasthan and in the district of Kutch in Gujarat.[18] These sites have been argued by numerous archaeologists in India to be on the banks of the mythic river Saraswati. This claim has been bolstered by recent research findings of geologists and remote sensing scientists who have suggested that the contemporary dry paleo-channels of Ghaggar-Hakra actually correspond to the mythic river Saraswati mentioned in evocative terms in the Rig Veda (see Gupta 2001; Ghose et al 1979; Yash Pal et al 1989; Joshi 1984; Misra 1994).

The SHP was proposed not just as an academic project. In order to justify funding from the Ministry of Tourism & Culture, in addition to the overtly presented research program, the project emphasized the transformation of fifteen archaeological sites into tourist attractions under the plan of 'Integrated Development of the Tourism Circuit from Adi Badri to

[17] The budget include money for the: "construction of archaeological complexes at 15 sites; exploration and excavation; conservation; horticulture and ecological up gradation; equipment; operational vehicles; library and related material; staff at the secretarial and hub Offices; consultancy and research activities; publication of reports; conducting seminars / workshops" (AACD, File No. 9/10/2003 - SH).

[18] The Saraswati has been identified with contemporary Hakra, Nara, Waihinda, Raini rivers in Pakistan and Ghaggar, Chautang and Drishadvati in India.

Dholavira.'[19] Of these, some of the hub sites like Thaneswar, Rakhigarhi, Banawali, Adi Badri, Aghora, Dholavira, Rangmahal, and Kalibangan had been previously excavated; Sirsa, Kalyat, Hanumangarh, Narayan Sarovar were local historic sites; whereas fresh large scale excavation was planned at the sites of Chak 86, Tarkhanwala Dhera, Baror, Hansi, Bhirdana and Juni Kuran. Jagmohan, the then Minister for Tourism and Culture, under whose tenure, the SHP was initiated, had located the project under his Rs. 300 crores, "Regeneration India" project, aimed at boosting "cultural and spiritual tourism" in India to exploit the domestic market. In a national newspaper, Jagmohan explained his justification as "Last year [2002] alone, domestic traffic increased by three crores. I have multiple objectives – to bring to life culturally significant monuments, towns, and sacred spots, improve the surrounding area and infuse keen civic sense to make it a pleasant experience. I also want to encourage visitors to come in contact with the profound minds which created all these wonders" (Gopinath 2003). However, at the heart of the SHP was a political project couched in academic, cultural, and tourist rhetoric—designed not to celebrate the Indian cultural landscape, but to establish the material manifestation of the mythic river Saraswati. This was a project of far-reaching consequences that utilized an invented Hindu assertion about ancient India to support the nationalist aspirations of the statist ASI with the scientific credibility of geology, hydrology, archaeology, and interpretation of Remote Sensing satellite data. The central objective of the project was to produce credible data of *indigenity* in order to scientifically demonstrate that the Rig Vedic Aryans were the authors of the Harappan civilization.

The SHP creatively and controversially attempted to bring together two of the most powerful imaginatives of ancient India under the rubric of one research schema – the Vedic Aryans and the Harappan civilization. Hitherto considered temporally separate, the Aryan people and the Harappan civilization, for the first time under SHP, have been made one under the investigative grasp of a statist project. The conceptual conduit for such commingling was the enigmatic and mythical river of Saraswati. However, this was not the first time that the Rig-Vedic Saraswati was being resurrected to rejuvenate the controversial proposition that the Aryans and the Harappans were homogenous cultural entities. The larger politics of the

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[19] Each of the site termed as a 'hub' was proposed to have an archaeological complex consisting of "an orientation centre; a documentation centre; a thematic library; a pavilion containing a model of the Saraswati basin in its cultural and topographical perspectives; dormitories for a short stay of research scholars, students as well as interested tourists. All above to be set in a garden setting with a pool of water symbolically representing the river(s)." (AACD, File No. 9/10/2003 - SH).

attempt was to substantiate the far more controversial claim that India was the original Aryan homeland and that Sanskrit was the Proto Indo-European language. Although in the official SHP proposal and its objectives, this larger subtext was never overtly stated, the SHP was genealogically located in the scholarship has been attempting to suggest over the course of more than eighty years that the Harappans are the original Aryans. This conceptual conflation was not a recent phenomenon; it dated back to the early nineteenth century when British colonial officials were traveling throughout the subcontinent and stumbled upon dry river beds in Western India. By the 1990s, the river Saraswati had become a central piece of a jigsaw puzzle called the Aryan debate, which was central to politics of the history of ancient India. The debate was about *indigenity* – about who were the original inhabitants of India, between the Aryans and the Harappans. Before I begin with my ethnography, it is important to take a detour to understand the complex historical and political context that the sites of SHP, in which I worked, were located in. The larger political structure of this debate did not have any substantial impact in the process and the practice of the ASI; however the political ramifications of the SHP did inform the interpenetrative imagination of the archaeologists who worked at these sites.

Conjuring Saraswati

The imagination of Aryans and Harappans has some weak epistemological foundations, often manipulated and exploited to produce incongruous and inconsistent theories about their origin, influence, and decline. The idea of a homogeneous Aryan people/race has been mostly based on linguistic evidence, where a formidable evidential consensus on the language has been established via Indo-European linguistics (Deshpande & Hooks 1979; Mallory 1989; Renfrew 1987; Parpola 1988; Trautman 1997, Bryant 2001; Erdosy 1995; Thapar 1996; Gamkrelidze & Ivanov 1995; Witzel 2001). However, the claim of the original Aryan homeland and the original proto Indo-European mother language continues to be dogged in controversies and marred with considerable speculations (Bryant & Patton 2005; Bronkhorst & Deshpande 1999). With it, the theories about dispersal and the movement of the Aryan people is also an epistemological can of worms (Anthony 1986, 1991, 1995; Parpola & Koskikallio 2000; Lamberg-Karlovsky 2002)[20]. This has led to resurgence in scholarship challenging the idea of

[20] Pit Grave culture(s) of the Pontic-Caspian steppe at 4000–2800 B.C., its descendant the Catacomb Grave culture(s) of 2800–2000 B.C., and its successors the Timber Grave (Srubnaja) culture(s) of 2000–1000 B.C. and the related Andronovo cultures of 2000–900 B.C. are some of the prominent

Aryan migration into India and arguing for indigeneity of the Aryans to India (Deo & Kamath 1993; Talageri 1993; Rajaram 1993; Rajaram & Frawley 1997; Frawley 1994; Elst 1999; Danino 1996). On the other hand, in the case of the Harappans, there is a sizeable archaeological record that provides rich evidence about their material existence, but virtually no concrete evidence exists about the language they spoke and their religious and social life. Only circumstantial evidence is available, which has led to a large scholarship based on conjectural and speculative assumptions (for instance Atre 1988; Jacobson 1988; Ratnakar 1991; 1992; Parpola 1994; Rao 1982). Since the discovery of the Harappan civilization, there has been a considerable *epistemological temptation* for a range of scholars in various disciplines to coalesce both the Harappan and the Aryan into one cultural whole. The endeavor has been to fit the literary, social, and the religious imagination of the Vedic Aryans with the monumental material manifestations of the Harappans, giving birth to the category of the Vedic Harappans (Shendge 1977; Gupta 1996; Singh 1995; Lal 2002; Bisht 1999; 2006). The SHP was a statist product of such an epistemological temptation - to force fit the varying pieces of *two* jigsaw puzzles in an attempt to make *one* conceptual whole.

If successful, Vedic-Harappans would manifest as an archaeological fact by potentially challenging one of the most fundamental theories in the last two hundred years of linguistic, archaeological, and Indological scholarship - the origin of the Indo-European language. It has been claimed that the Aryans who spoke the various Indo-European tongues spread from a Central Asian homeland to the rest of the Old world. However, if the Vedic-Harappans are indeed a fact, then this would undermine the Central Asian home-land theory and make India the original Aryan homeland and Vedic Sanskrit the original proto-Indo-European mother language. The ramifications of such a possibility – political, social, and religious – are immense. This postulation has been challenged by numerous scholars and has led to fierce and publicized debates in recent times (Bryant & Patton 2005), with its most acerbic confrontation obvious on the Internet. Although not explicitly articulated in the SHP objectives, the project was ASI's attempt at participating in this so called 'Aryan debate' and designed to generate archaeological data to close the controversy.

It was Cunningham who is credited to have recognized the archaeological potential of

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cultures that archaeologists claim to be material evidence of the Indo-Aryans people. However there is no consensus on their relationship to each other (Lamberg-Karlovsky 2002)

Harappa in 1872-73; however the significance of the discovery eluded him (see Pande, 1982; Possehl, 1982 Singh 2004; 97-98; Lahiri 2005: 1). Sir John Marshall, in his 'breaking news' article in 1924, about the Harappan Civilization in the *Illustrated London News*, underscores the importance of the discovery:

> Up to the present our knowledge of Indian antiquities has carried us back hardly further than the third century before Christ...now, however, there has unexpectedly been unearthed, in the south of the Panjab and in Sind, an entirely new class of objects which have nothing in common with those previously known to us, and which are unaccompanied by any data that might have helped to establish their age and origin"(Marshall, 1924: 528).

With this situation was born the enigma of the Harappan civilization. Since then, it has had a privileged epistemological status in Indian archaeology - closely tied to the meaning of Indian civilization and the cultural politics of the nation's ancient past. The source of the historical importance of the Harappan civilization lies in the fact that it has pushed the chronology of the civilizational history of India back by two thousand years. Prior to the discovery, the Aryans were credited to be the original settlers of the subcontinent. The abrupt appearance of a pristine ancient civilization, of the magnitude and sophistication of the other known civilizations of the old world, spurred a long and still ongoing process of the rewriting of Indian antiquity (see Panikkar 2001; Sarkar 2002; Thapar 2001, 2005; Guha 2005). In its early years, the production of knowledge about Harappan civilization was closely interlinked with the colonial ideology of imperialism and carried the subtext of a civilizing mission. After independence, the Indus civilization has been deeply associated with the rise of Indian nationalism (see Guha-Thakurta, 2005; Ramaswamy, 2001; Chadha 2002; Chakrabarti 2003; Bhan 2000; Thakran 2001; Guha 2005)[21].

The Neolithic Mehrgarh Period I- IV (7000 – 3500 BCE) has been argued to be the precursor to the Harappan civilization, but it is with the Ravi (3200-2800 BCE) & Kot Diji (2800-2600

[21] It is important to note here that throughout this dissertation I use the term "Harappan civilization" to denote what have been usually called the Indus Civilization or the Indus Valley civilization. Most recently it has been called the Indus-Saraswati civilization, Sindhu-Saraswati civilization and even just the Saraswati civilization – each of these names have certain political and ideological genealogy. Naming of the Harappan civilization has been fraught with controversy since Marshall initially called it the Indo-Sumerian. Therefore I have decided to follow the standard practice in archaeology by calling it by the first type-site excavated – Harappa. Ernst Mackay was the first to use this terminology (Mackay 1938: 39).

BCE) phases of the early Harappans that the cultural traits of the civilization distinctly emerge. The mature urban phase of the major sites, dated between 2500-1900 BCE, gave way to the decaying late post-urban Harappan phase. Extensive archaeological evidence suggests that the Harappan civilization was declining by the early centuries of the second millennium BCE. By 1400 BCE, most cultural traits associated with the Harappan civilization had either disappeared or withered away – the monumental architecture of the Harappan cities had given way to inferior settlements and the characteristic seals stop making appearance in the habitational deposits. Although there is no unanimity about the causes of the decline of the Harappan civilization, it is widely regarded to have dissipated as a homogenous cultural marker in the archaeological record by this time. (see Chitalwala 1985; Bhan 1989; Possehl & Raval 1989; Allchin 1995; Alluchin & Joshi 1995; Dhavlikar, Raval & Chitalwala 1996). And it is around the same period that the Aryans are supposed to have entered into the Indian subcontinent.

The nineteenth and early twentieth century historical narrative of ancient India was very closely associated with Indo-European Aryans. Colonial Indologists had created a historical narrative attributing to the Aryans the birth of Indian civilization (see Trautmann, 1997; Chakrabarti, 1997). Right from the celebrated discourse by William Jones in 1786, which led to the birth of the 'Aryan race' and 'Indo-European linguistics,' until the discovery of the Harappan civilization, it was widely believed that Aryans entered into India from the west and inhabited the subcontinent for the first time. By the1920s, the Aryan theory had evolved as a racial theory of intrusion, built on more than a century of comparative philology and ethnology. The fundamental assertion of this theory was that the Aryans were a superior racial group that occupied the river valleys of western India and the Gangetic plains by vanquishing the weaker indigenous tribes and creating colonies (Trautman 2000; Bryant 2001; Chakrabarti 1997; Leach 1990; Thapar 2001). However, the date of the arrival of the Aryans is an exploratory area of scholarship principally based on the philological analysis of the internal structure of the Rig Veda. We do not have any absolute dates; only relative dates are available, and these fluctuate between 1700 BCE to 1100 BCE (Deshpande & Hooks 1979; Mallory 1989; Renfrew 1987; Parpola 1988; Bryant 2001; Erdosy 1995; Thapar 1996; Gamkrelidze & Ivanov 1995; Witzel 2001). It is widely assumed that the authors of the Rig-Veda, around this time period, were in the geographical area of the '*Sapta-Sindhu*,' – the land of seven seas geographically located in Northwestern India in Punjab. Temporally, therefore the period

between 1700 BCE and 1300 BCE becomes significant, because it is only in this small time slot that the Harappans and Aryans could have overlapped. It was Mortimer Wheeler, who for the first time, attempted to archaeologically fix this contact, albeit as a violent confrontation - a theory already in currency since 1926 when Gordon Childe notably stated that the "Aryans were just the destroyers of the newly discovered culture [Harappan]" (Childe 1926: 34). Consequently, in his report on the excavation in Harappa, Wheeler notoriously adjudicates:

> Climatic, economic, political deterioration may have weakened it [Harappan civilization], but its ultimate extinction is more likely to have been completed by deliberate and large-scale destruction. It may be no mere chance that at a late period of Mohenjodaro men, women, and children appear to have been massacred there. On circumstantial evidence, Indra stands accused" (Wheeler 1947:82)

Wheeler came to these conclusions after the exposure of the skeletons of the cemetery "H" culture in Harappa. The objective valence of his claims was categorical for he was able to summon not just the archaeological context of the discovery but also successfully employ anthropometric criteria to reinforce his premise. Although dispelled by Marshall as knotty (Ramaswamy 2001; Guha 2005), the proposal arguing for anthropometric similarities between the skeletal remains in Mohenjodaro and the southern Indian Dravidian gained legitimacy, giving rise to the theory of the Dravidian origin of the Harappans. Both these theories attempted to provide a scientific bulwark to the narrative of the invading Aryans in their horses and chariots who decimated the Dravidians and pushed them south. Wheeler's theory was subsequently discredited (Dales 1964; Kennedy 1994), and the myth of the Aryan invasion was tempered into the idea of the Aryan migration – a diffusion model of migrating Aryans interacting with indigenous settlers (Allchin & Allchin 1997; Jha 1998; Sharma 1999; Thapar 1984).

In addition to this temporal moment of interaction, the question of the physical location of contact has been at the centre of Aryan-Harappan debate. It is here that the mythic Saraswati comes into play. Until 1947, major archaeological sites of the Harappan civilization had been chiefly located on the banks of the river Indus; however following extensive exploratory work in Haryana, Rajasthan, and Gujarat in Western India, more than 1500 sites were discovered in India by the 1990s (Thakran 2000)[22]. All these sites were at a considerable distance from the

[22] Till 1947 less than 40 Harappan sites were known (Wheeler 1953:95-96) of which only two were in India - Kotla Nihang Khan (1929-30) in Punjab and Rangpur (1934-35) in Gujarat both which were

catchment area of the Indus (between 200 to 500 kilometers away) and were located in the arid zones of Saurashtra and Kutch in Gujarat and the semi arid region of northern Rajasthan and Haryana. Large clusters of these sites were situated on the dry paleo-channels of Ghaggar-Hakra in India. This discovery and subsequent archaeological excavations[23] were responsible for the birth of the ides of the Vedic Harappan and the plea to change the name of the Indus Civilization to Indus-Saraswati Civilization was made. However, this was not the first time that there had been an attempt to archaeologically identify the Aryan with a specific material culture. Aryans had been earlier associated with Gandharan Grave culture, Cemetery 'H' culture, Banas Culture, Malwa culture, Chalcolithic culture of western and northern Deccan, Copper-hoard culture, and Painted Grey Ware culture (Thapar 1970: 147).

For the seers of Rig-Veda, the river Saraswati was "*Ambitame, naditame, devitame*"- the best of mothers, the best of rivers, and the best of goddesses (RV II.41.16). As this quotation suggests, the term Saraswati epitomized both a riverine body and a magnanimous feminine divinity – a polysemic characterization that in the complex etymological universe of Vedic Sanskrit can have multiple meanings and connotations depending on the context of its usage. One frequency analysis show that the term occurs around sixty-eight times in the Rig Veda, more than fifteen times the frequency of the term Sindhu (the contemporary Indus) - the other riverine body that is often described with similar epithets in the Vedic lore (Singh 1998). Eloquently and evocatively described in the Rig-Veda as a grandiose river, the Saraswati is often referred to as that water body that rushed:

> From the mountains towards the ocean, VII Mandala, 95th Sukta, 2 verse (VII.95.2) with tempestuous roar (VI.61.8) breaking and carrying down ridges of hills like lotus stems (VI.61.2), filling the realism of earth and wide tracts of the firmament (VI.61.11). She is far superior to her companion (VII.95.4) and surpasses all other streams by her sheer majesty (VII.95.1) and glory (VI.61.13). This is fierce (VI.61.13), swiftest of the swifts (VI.61.13), mightiest of her class (VII.96.1)" (Gupta 2001:30)

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excavated by M.S. Vats. By 1984, 1400 Harappan sites were discovered due to extensive exploration efforts of the Archaeological Survey, the departments of Ancient Indian History, Culture and Archaeology in the Universities of Kurukshetra and Baroda, Deccan College, and the State department of archaeology in Gujarat (Misra 1994: 511).

[23] Archaeological excavations of some of these sites were carried out by the ASI in the 1960s and 70s – prominent amongst them were Kalibangan (Rajasthan), Lothal, Surkotada (both in Gujarat).

Although most of the rivers mentioned in the Rig Veda - Indus, Sutlej, Yamuna, Ganga have their terra-firma manifestations, it is Saraswati whose earthly embodiment is absent. In the latter Vedic texts like the *Satapatha Brahmana, Aitareya Brahmana* and the *Jaiminiya Brahmana*, the magnificent Saraswati of the Rig Veda is described as a disappearing river which had shrunk in size and virtually disappeared. In the Mahabharata, the river is described as drying up in a dessert. In later *Purnaic* lore, the Saraswati is referred to as the subterranean river that eventually resurfaces at the *triveni sangam* in present day Allahabad where the two of the most sacred rivers of Hinduism merge - Ganga and Yamuna. It was this enigmatic manifestation of Saraswati that has enamored geologists and archaeologists since the late nineteenth century, when these disciplines gained influential scientific authority in the cultural landscape of colonial India.

The search for this "lost river" began with the interpretation of the much celebrated *nadi-sukta* (RV: x.75) by a French Indologist, M. Viven de Saint-Martin, who in 1860 suggested that this hymn "must have been composed, or technically *seen* (revealed), after the arrival of the Vedic Aryans on the banks of the Saraswati" (Thomas 1883: 363; italics in the original). By the late nineteenth century, articles about the "lost river of the Indian dessert" made appearances in Indological journals and hypotheses about various dry riverbeds in Western India being the Rig-Vedic Saraswati were being postulated (see Oldham 1893; Oldham 1887; Nearchus 1875; Maclagan 1885; Raverty 1892, Wilhelmy 1999[1969]). The discussion in these articles was centered on the paleo-channels of Hakra and Nara and their hydrological relationship to Indus, Yamuna, and Sutlej in the present regions of Sind, Punjab, and Rajasthan. These rivers had been the objects of surveys and travels by various colonial officials in the early nineteenth century (see Baker 1840; Burns 1835; 1973 [1834]; Murdo 1834; 1939). The resurgent interest in revisiting the dry riverbeds of Hakra and Nara was caused by burgeoning interests in mapping the geography of ancient India, influenced by the works of Alexander Cunningham by the latter half of the nineteenth century (Cunningham 1871). Most of these articles provided incongruous views, however a general consensus seemed to be that the Hakra-Nara system must have been a formidable hydrological body when the Aryans arrived, but the association with Rig-Vedic Saraswati was, at best, speculative in these articles.

With the discovery of the Harappan civilization in the 1920s, the scholarly interest in the Aryans and the Rig Vedic period subsided, as archaeologists were more attracted in

correlating the Harappan material culture with other western Bronze Age civilization. The decade of the 1920s saw spectacular discoveries in Egypt and West Asia with the opening of Tutankhamen's tomb in 1922; the library of cuneiform tablets and a palace from Kish in 1924: royal cemeteries in the Ur of Chaldees in 1926.[24] Thus, for Indian archaeologists who were investigating Harappan culture, it was natural to co-relate the Harappan material culture with the west rather than compare it with literary records in the Vedic literature. For example, Rakhaldas Banerji was comparing artifacts found in Mohenjodaro with Minoan artifacts in 1923, and within weeks of the publication of Marshall's announcement in 1924, it seemed confirmed that Harappans were in contact with the Mesopotamian civilization (see Lahiri 2005; Guha 2005). The Aryans, who the colonial historians had politically subsumed within their racial ideology (Trautman 1997), were further eclipsed because the Harappan civilization, in the public imagination of a nation resisting colonial domination, provided India with a chronology that predated the Aryans by at least a millennium. Evidentially, the historicity of narratives in the Sanskrit literature was always questionable. More often than not, the Greek sources, both literary and numismatic, were summoned to delineate the chronology of ancient India. Thus, the discovery of a formidable archaeological record of the Harappan civilization overshadowed the already vulnerable historicity of Sanskrit literature and consequently reduced the scholarly obsession with the Aryans.

Saraswati had not been completed obliterated from the world of scholarship. In 1927, a German scholar of Indo-Iranian argued that the term Saraswati was a cognate of old Iranian *Harahuvatii*, and referred to a cosmological water divinity *Aredvi Sura Anahita* in the Avesta (Lommel 1927: 1954). This led to the identification of *Harahuvati* with the Helmand River in Afghanistan, and by default to Saraswati. This was also a conjectural hypothesis and did not find many takers –Helmand does not enter the sea, but drains into a marshy area –nonetheless it is still a theory that is ardently pursued by some (Kochaar 1999). In 1942, Sir Aurel Stein, a British political agent, explorer, and archaeologist, who had been associated with the ASI since early 1900s and had conducted extensive archaeological explorations in Western India and Afghanistan famously declared that the Ghaggar-Hakra paleo-channel was indeed Saraswati (Stien 1942). With it, began the next phase of co-relations of dry rivers beds with

[24] These discoveries were sensationally announced in the *The Illustrated London News* (Guha 2005: 406) - the location not surprisingly, also chosen by Marshall to report the discovery of the

Saraswati, propelled by archaeological discoveries of the Harappan civilization in 1924. Between the 1950s and the 1970s, the discovery of a large number of Harappan sites in the Ghaggar-Hakra area made Vedic Harappan, a tantalizing possibility. This exploration was a follow up to Stein's work and was a reaction to compensate for the loss of nearly all the Harappan sites to Pakistan, and the need to re-establish the pre-eminence of post-independence India as an ancient civilization (Ghosh 1952; 1959; Bhan 1973; Thakran 2000; Guha 2005). These were not mere nationalistic attempts to replenish the losses of partition, but had the larger goal of destabilizing the locus of the Harappan civilization away from the Indus valley to a larger geographic area - as far as the Gangetic plains in the east to the Saurashtra in the South[25]. By 2000, of the 2317 Harappan sites known, 1474 were in India and 1074 were located on the Ghaggar-Hakra – 426 in Pakistan and 648 sites in the Indian part of the dry riverbed. These discoveries, along with the excavations of the Harappan sites of Lothal, Kalibangan, Surkotada, Bhagwanpura, and Banawali over the decades, rekindled the debate of the possibility of the existence of the Vedic Harappans. The presence of horse bones in Surkotada (Joshi 1990); the interpretation of the Harappan fire hearth as the Vedic fire-altar at Kalibangan (Lal *et al* 2003) and Banawali (Lal 2002), Lothal (Rao 1985); the representation of spoke wheels on terracotta toys in Kalibangan, Banawali (Lal 2002) along with several alleged decipherments of the Harappan script as proto-Sanskrit (Jha & Rajaram 2000; Rao 1982; Shendge 1997); were all taken to strengthen the theory of the Vedic Harappans. By the late 1980s, with the rise of Hindu fundamentalism, the project of the "Aryanization of the Indus civilization" was undertaken by historians and archaeologists closely associated with the Right Wing Hindutva political parties (Bhan 1997: 13; 2000; Thakaran 2000: 62: Guha 2005). This project simultaneously argued against the migration of the Aryans from the west and for the indigeneity of the Harappans (Rajaram 1995; Elst 1999; Talageri 2000).

The hydrological centre of this process of 'Aryanization' was the river Saraswati, which over the century, had swiftly moved from the mythological to the archaeological realm, and then had been reborn via postcolonial geological sciences. The work of geologists, with interpretative reading of remote sensing satellite data, and paleoclimatic, paleoseismic research, led to scientific claims establishing the co-relation between Ghaggar-Hakra and Saraswati. In 1979-80, invoking the *nadisukta* hymn and earlier hypotheses from Oldham to

[25] Rafique Mughal's exploration in eastern Pakistan adjoining the Thar dessert was also responsible for this shift (Mughal 1997).

Stein, a group of geologists and archaeologists interpreted satellite imagery taken between 1972-77. They validated the century old speculation that Ghaggar-Hakra had indeed been a large river and forcefully argued that the ancient paleo-channel was the Rig Vedic Saraswati (Ghose *et al* 1979; Yash Pal *et al* 1989; Baliwal & Grover 1988; Radhakrishna & Merh 1999; Roy & Jhakkar 2001; Ramaswamy 1988; Ramaswamy *et al* 1991; Valdiya 2001; Kar 1999, 1984; Kar & Ghose 1984; Puri 2001). This was the first introduction to an entirely new set of data with significant scientific credence – 'Landsat' satellite imagery. These announcements, and archaeological findings of a large number of Harappan sites in the Ghaggar-Hakra region (Mughal 1997; Thakarn 2000) were responsible for the resurgence of Saraswati as a archaeo-geological entity. Overwhelmed by the satellite data as well as the high concentration of Harappan sites in the Ghaggar-Hakra region, archaeologists contentiously argued that it was Saraswati rather than Indus that was the centre of the Harappan civilization (Joshi 1984; Misra 1994). This also led to uncontested assertions about the links between the Ghaggar-Hakra and the Saraswati, and by association, with the Harappan cultures (Lal 2002; Gupta 1995, 1996, 2001; Kalyanraman 1999). Incestuous citing practices on the part of like-minded geologists and archaeologists created a perception that the Saraswati River could indeed be treated as an emergent objective scientific fact. The rhetoric of Vedic Harappans, coupled with the critical rebuttal of the Aryan invasion, the pronouncement of Ghaggar-Hakra, and the absence of a credible explanation for the collapse of the Harappan civilization, all contributed to the gradual emergence of the Indus-Saraswati civilization as an unquestionable factual reality (Radhakrishna and Merh 1999).

However, on closer reading of the papers published by the geologists, it seems that there were contradictory claims circulating, not just about the provenance of the river, but also its nature. Some argued that the Ghaggar-Hakra was a perennial river, others said that it was fed by Yamuna and Sutlej, while others stated that the river was monsoon fed (see Radhakrishna and Merh 1999). Theories about its disappearance also abound – from its demise due to the changing of the course of Sutlej and Yamuna to seismic activity in the region, which made it subterranean, to its drainage in the Kutch. The only consensus that seemed to have emerged that Ghaggar-Hakra had indeed been a powerful hydrological body. No confirmed temporal evidence was available to help pinpoint when the river dried up or whether it finally drained into the sea or the dessert of Thar. Recent paleoclimatic researches have shown that aridity had set in Thar by 4800 BC, nearly a millennium before the mature phase emerged in the

major urban centers of the Harappan civilization. This suggests that the culture must have risen and fallen in semi-arid climatic conditions similar to present day (Enzel *et al* 1999). Another recent study of the isotopic content of the alluvium of Ghaggar-Hakra suggests that the hydrological content of Ghaggar-Hakra did not originate in the Himalayas, thus contesting the very idea of the perennial Saraswati (Triphati *et al* 2004). These findings question the fundamental theories of the hydrological nature of Saraswati river, and suggest that the Harappan culture flourished at a time much later than the time of the mighty Ghaggar-Hakra. More research is imminent in this area of paleoclimatology and hydrology to determine the facts about the demise of the Ghaggar-Hakra. Archaeological and geological research has shown beyond doubt that a significant portion of the Harappan civilization was situated in the Ghaggar-Hakra region, but it is not clear if this region was already affected by aridity when the Harappans settled there or if it was still a mighty river, as the proponents of the Saraswati seem to argue. However, with the rise of Hindu fundamentalism in the early 1990s, and their finally ascendancy to power on the national level in 1998, politicians, Indologists, archaeologists, and geologists who were politically invested in the idea of Saraswati civilization were able to plan and configure the SHP.

What was significant about the SHP was that for the first time, a state-sponsored project was instrumental in trying to investigate the relationship between Ghaggar-Hakra, Harappan culture, Rig Vedic literature, and Saraswati. Senior archaeologists of the ASI, mostly retired and some still serving, who had earlier articulated the idea of Vedic Harappan, were at the forefront of the conceptualization of the SHP and were members of the ASI instituted "Advisory Committee for the Multidisciplinary Study of River Saraswati." However, some of these members had very intimate connections with the Hindu nationalist political organization - thus even before the excavation began in 2003, the SHP got embroiled in controversies about the ideological impetus of the project. This role of the ASI and the government turned SHP into a politically powerful project, and this was eventually the reason for its premature end. The excavations under the SHP lasted only a single season – 2003-04, when the National Democratic Alliance (NDA), under the leadership of the Hindu nationalist Bharatiya Janata Party (BJP) was in power. In the general election in the summer of 2004, NDA was defeated and the United Progressive Alliance (UPA), under the leadership of the centrist Congress and the support of the left parties, came to power. During BJP's tenure the SHP had received criticisms from archaeologists and scholars who were not sympathetic to the political subtext

of the SHP, among these were prominent scholars, who were close to the left parties. Jaipal Reddy, the first Minster of Tourism and Culture under the UPA, officially scrapped the SHP by the fall of 2004 and funding was cut. A high power Parliamentary Committee was set up when the UPA government came to power in 2004, with the explicit aim of investigating the working of the ASI (Basu 2005). This committee was formed at the behest of the left parties, who were a significant partner of the UPA government, with an implicit focus of investigating the SHP. The "91st Report of the Department-related Parliamentary Standing Committee of Transport, Tourism and Culture" devoted to the functioning of the ASI, severely indicted it in the context of the SHP:

> After going through the replies furnished by the Ministry of Culture, the Committee is of the firm view that Saraswati Heritage Project did not conform to the criterion fixed for excavation of archaeological sites since no academic body or university had recommended the project...The Committee further notes that the Ministry is not clear as to which research agency/scientific survey actually pointed out that the dry beds of River Ghaggar and River Chautang (River Drishadvati) are the beds of River Saraswati. The Committee understands that the existence of River Saraswati is purely a mythological one and a scientific institution like Archaeological Survey of India has not correctly proceeded in the matter...The Committee would like to advise the Archaeological Survey of India that it should prevent itself from taking up exercises without a scientific basis which have all potentiality for subjective interpretation of historical facts thereby, leading to controversies (Basu 2005: 13)

Thus for the season 2004 –05, excavations ceased at the sites of Chak 86, Tarkhanwala Dhera, and Juni Kuran, sites which were being excavated by the Bhubaneshwar Ex. Br and Vadodara Ex. Br. respectively. However Nagpur Ex. Br, Delhi Ex. Br, and Patna Ex. Br continued excavations at Bhirrana, Hansi, and Baror respectively for the season 2004-05. Although the SHP as a political project of the ASI met with an untimely end, its historical and ideological foundations have not withered; they still continue to occupy a significant space in the archaeological imagination of ancient India.

I conducted an ethnography of the ASI sites both during its excavation under the ideological and financial auspices of the SHP in 2003-04 and also after the SHP was terminated in 2004-05. Although I observed that there was no marked impact on the daily practices and process of the ASI, the ideological overview of the Aryan debate, the possibility of the Vedic Harappans

and the Ghaggar- Hakra as Vedic Saraswati were regular points of discussion in conversation with my informants. Every archaeologist I spoke to was aware of the political valence of the SHP but it did not have any impact on their archaeology, which, I show in this dissertation, was framed by far more powerful ideologies and organizational formations. Nonetheless, I think it is important to preface the ethnographic account of this dissertation with the political and nationalist subtext of the archaeological excavations I worked in. The ASI was essentially a postcolonial governmental organization, and it's archaeological intervention, like any other nationalist organization anywhere in the world, was framed by the political climate of its location. However, as a statist organization its authority emerged from its professional subjectivity as a neutral intervener of the state beyond petty political and ideological reverberations. Thus, as a statist organization, it was not colored by such ideological projects but attempted to function under its shadow.

Descriptions of the Archaeological sites

The sites that I did ethnography in, although subsumed under the political-epistemological research umbrella of the SHP, were fundamentally Harappan sites. These were sites that had been discovered during extensive exploration work since the 1950s - of which the ASI excavated Dholavira (1984-85, 1989-90 through 2004-05), Rakhigarhi (1997-98 & 2000-01), Lothal (1984-85, 1959-60 & 1961-63), Surkotada (1970-72), Kalibangan (1960-69), Kunal (1985-86, 1991-92, & 1993-95), Bhagwanpura (1975-76), Banawali (1983-84, 1986-87 & 1987-88) to name the most prominent. One of the important thrusts of the SHP was further excavation in these areas. The sites of Juni Kuran, Hasi, Baror, Bhirdana, Chak 86 & Tarkhanwala Dhera were chosen for fresh excavation, most of which had been discovered a few decades before. None of the official documents explain why these sites were chosen for excavation under the SHP, and only state that the ASI, on the recommendations of the Advisory Committee for the Multidisciplinary Study of River Saraswati, had planned to excavate these sites. The research objectives about each of the sites were also not specific, for example, in the case of Juni Kuran which belonged to the Gujarat enclave of the Harappan civilization, its excavators state that the objectives of the archaeological excavation was:

> (a) To define the River Sarasvati and its tributaries in the basin; (b) to identify special items of geo-technical studies; (c) conducting archaeological research to know the cultural sequence and specialized studies on ceramics, metallurgy, mineralogy, botanical and zoological; remains; (d) and to confirm the paleo-channel of the 'lost'

Sarasvati river, as per Satellite Imaginary Data" (Pramanik 2004: 46).

In a similar light, the objectives of the Bhirrana excavations were specified as: "1) Determining the regional identify of the Harappans in the Saraswati river Valley; 2) understanding the cultural sequence and chronology of the site; and 3) Settlement pattern of Early Harappans in the Saraswati river valley" (Rao, et al 2004; 20). Thus the excavations did not have specific objectives beyond the customary cultural history emphasis on unearthing cultural sequences on each site and placing it within the larger material and temporal chronology of the Harappan civilization. The idea that these sites were on the mythic Saraswati was a 'fact' taken for granted and the excavation objectives were to primarily delineate the archaeological history of each of these sites. Below is a brief description of each of these sites.

In search of possible land-routes connecting the Harappan epicenter in Sind with the outlying province in Gujarat, Jagat Pati Joshi in 1967-68, then an SA in the ASI, discovered, among others, the enormous Harappan site of Dholavira in the district of Kutch (Taluka Bachau).[26] Preliminary exploration work at the site was done in 1984-85, however excavations only commenced in the year 1998-99, undertaken by R.S. Bisht the then SA of the Vadodara Ex, Br. 2003-04 was Dholavira's twelfth season of excavation. During these years, R.S. Bisht rose in rank from the SA of the Vadodara Ex, Br. to the Director of the Institute of Archaeology, was transferred as the Director of the Exploration & Excavation department in the DG headquarters in Delhi, and eventually became the JtDG of the ASI.

As a result of the ASI excavations, Dholavira has emerged as the fourth largest Harappan city, remarkable for its exquisite planning, monumental structures, aesthetic architecture, and an elaborate water-management system.[27] The centre of the huge 100 hectares site is a fortified township consisting of the citadel, the middle town, and the lower town- so named by archaeologists on the basis of their relative location, layout, and architecture. The citadel and the middle town are heavily fortified with elaborate defense walls while the lower town does not have any such fortification of its own, although it is encapsulated by the larger fortification of the town. The citadel is located in the south and it said to have two conjoined subdivisions

[26] The ruin of the settlement is locally known as ' Kotada' (Lat. 23 53' 10" N, Long. 70 13' E), lies one kilometer NNW of the village of Dholavira.

[27] Dholavira is largely regarded as the fifth largest Harappan settlement, following Mohenjodaro, Harappa, Ganweriwala (in Pakistan) and Rakhigarhi.

– the castle on the east and the bailey of the west. Surrounding this main fortification is a series of reservoirs, part of the elaborate water harvesting system in Dholavira fed by seasonal streams. Dholavira also yielded an inscription of ten large-sized signs of the Harappan script, which the excavators argue was probably the oldest sign-board in the world. Through the enormous accumulation caused by successive settlements of over a millennium, the archaeological excavations over the course of twelve seasons have revealed seven significant cultural stages (numbered from Stage I to VII) of the rise, culmination, and fall of the Harappan urban system. Stages I & II have been identified with Pre-Harappans, who were instrumental in constructing a strong fortress and have temporal and cultural affinity with Amri IIB, Nausharo ID, and Kot-Diji. Stage III has been called the "most creative and important" stage during which expansion and extension occurred and it was the immediate precursor to the Stage IV - the stage of "classical Harappan culture" (Bisht 1999: 29). The subsequent stages have been designated as marking the decline of the Harappans. Other than the archetypal mature Harappan architectural characteristics of planned township and drainage systems, one of the most important discoveries of Dholavira has been an elaborate water conservation system consisting of sixteen reservoirs of varying sizes (Bisht 1999: 27). The excavation work at Dholavira during the season I was at the site involved exposing the fourth corner of the Eastern Reservoir, and inconclusive features of Southern Reservoirs, which include (1) the Castle where a well-plastered mud wall of Stage III was exposed, and the Burial Mound 2, where altogether new characteristics of sepulchral architecture are awaiting logical exposure.[28] For the proponents of the Vedic-Harappans, Dholavira emerged as an important monumental manifestation of the Indus-Saraswati civilization, and by 2003-04, it was considered to be the most important site of the SHP along with Rakhigarhi.

The site of Bhirrana, located in district Fathebad in Haryana, was discovered in 1982. Before having been declared a protected monument, it was a Muslim burial ground primarily in use before partition. The excavations at Bhirrana, were being conducted by the Nagpur Excavation Branch, which was the first excavation branch of the ASI, and it had the reputation as one of the best. The mound, located at the edge of the village of Bhirrana, was supposed to be "overlooking the left bank of the now dried up river Saraswati" (Rao, et al 2004; 20), or the

[28] The Dholavira excavation budget for that year was estimated to be Rs. 43.30 lakhs (AACD, File No. F.15/30/2003-EE).

Ghaggar. The first season of excavation suggested that the site consisted of three distinct periods - early Harappan, transitional, and mature Harappan. The characteristic feature of the early Harappan settlement that the excavators found was circular pit dwellings, which had also been reported at the sites of Mithathal and Kunal in the adjoining region. The Period II, which has been designated as a transitional period, was identified with the fortification of the site. These periods were followed by a Mature Harappan phase with distinctive town planning and material cultural assemblages. The objectives of the excavation during the season I was working (2004-05) were elaborated as:

> 1) a detailed study of the town planning of the mature Harappan phase; 2) to identify the dividing line between citadel and the lower town; 3) to locate the extent of fortification wall towards north, south, and east; 4) to study the real nature and purpose of the circular structures found within habitation; 5) an intensive study of the early Harappans in view of a considerable deposit of 1.70 mts, and to know the nature of the pit dwellings found in this region (Rao 2004).

The excavation at Bhirrana continued until the season of 2005-06 and the Carbon 14 dates of the early Harappan layers has brought it considerable limelight because the site has been dated to have the earliest Early Harappan layers to be found in India.

The site of Baror is located about 100 km southwest of Kalibangan, the famous Harappan site. Excavations have unearthed the remains of the pre-Harappan and the mature Harappan cultures. Baror was similar to Bhirrana. Prior to being declared protected, it was used as a Muslim burial ground and was under the ownership of the local Wakf Board. The Patna Ex. Br had undertaken excavation at this site, and I worked at the site for two seasons: 2003-04 and 2003-05. This was a site which also had both early Harappan and mature Harappan layers.

As part of the SHP, the Delhi Ex. Br. was asked to excavate the sites of Pilibangan, near the famous Kalibangan. However during the course of my fieldwork I came to know that excavation at Pilibangan was abandoned because, although this site had been designated as protected for the last few decades, it had been used as a Muslim graveyard, and belonged to the Wakf Board. The SA of the Ex. Br. had allegedly refused to excavate this site as it would cause communal tension. Instead, the Delhi Ex. Br, was asked to excavate the mammoth multi-cultural mound of Hansi. This site, located near the dry bed of river Drishdavati, was essentially a medieval fortification, which had been occupied by the Rajputs and subsequently

by the Mughals and finally by the local British colonial officials. Unlike other SHP sites, this was not known to be a Harappan site, although the explicit objective of the excavations was to locate the Harappan layers. During the two seasons that I was present, a few vertical trenches were excavated but no Harappans layers were found.

The bulk of the ethnographic accounts in this dissertation have been collected during my stay at these excavation sites and camps between 2003-05. During these two years, I spent a considerable time working at the ASI archives in the DG headquarters in Delhi, the National Archive, Deccan College Library, Pune, Asiatic Society of India Library and Archive, Calcutta and the India Office Library, London.

Ethnographic Methodology

My ethnographic intervention is informed by two methodological techniques in the sociology of science which have gained importance over the last two decades. The first is the phenomenological approach epitomized by *enthnomethodology* (Garfinkel 1967, Lynch 1993) and the other is the mode of *anthropological ethnography* (Knorr-Cetina 1981, 1999, Latour 1986, 1987, 1999, Traweek 1988, Rabinow 1996, Helmreich 1998, Gusterson 1998, Rapp 1999). The focus of both techniques has been to analyze how scientific method works and creates knowledge, with an intense scrutiny on a single location of its performance. In the case of my research, it will be the archaeological site. Ethnomethodology studies ways in which members of a cultural system, through their practices, produce the social structure of everyday activities. Its aim is to explicate these ways of meaning production and it attempts to describe those practices and show how they work (Garfinkel 1967). Ethnomethodology has been successfully utilized as a technique by numerous sociologists of science to study the working of science (Lynch 1985, 1993, Woolgar 1988a, 1988b, Yearly 1984). Its investigation is directed to uncover social processes underlying the construction of social phenomena ranging from factual knowledge, social organization, and attributes such as race and gender, to the acquisition of tacit skills (Polanyi 1958).

Postcolonial bureaucratic institutions are virtually closed systems. Getting access to individuals and obtaining permissions requires maneuvering the bureaucratic machinery not just for an outsider like me, but even for a government employee. This is a daunting task involving enormous patience, lengthy paper trails, inconsiderate superiors, indifferent

orderlies, and a rigid system that is often corrupt and always difficult to negotiate. Bureaucratic machinations are at work constantly and each and every instance of negotiation is met with resistance and opposition and no one is spared. As an elderly informant very grimly remarked "everyone gets crushed" [*sab piss jate hai*]. In such a situation, it was not a simple task to penetrate the ASI and do ethnographic work. It would have been impossible to get official permission to work at the excavation sites if I had exposed my true "ethnographic self" to the opaque bureaucratic system, suspicious and apprehensive of outsiders. In order to physically reach the ASI archaeological sites and do immersive ethnography I had to negotiate with two forms of bureaucratic red tape. The first involved getting official permissions "in writing," from the DG headquarters in New Delhi to be able to go to an archaeological site and the second dealt with the explaining of my research objectives and the nature of ethnographic intrusion to my informants at the ASI sites. Both required different strategies.

While I was in the process of conceptualizing my project and writing my first grant proposal for the American Institute of Indian Studies (AIIS), to get funding to do this ethnography, one of my advisers cautiously suggested, "do not write about ethnography or anthropology of the ASI. The Home Ministry in Delhi that will finally recommend your proposal, will be suspicious. Frame it as a history of archaeology in India". I greatly value this advice, as it saw me through my fieldwork. Even Latour & Woolger in their introduction to "Laboratory Life," at the outset, underscore that the scholarly "label" of a historian or a philosopher is more successful in subduing the participant's sense of suspicion than the disciplinary grasp of anthropology or sociology, which have more intrusive emphasis (Latour & Woolger 1986 [1979]: 20). Heeding my adviser's suggestion, for all official permissions to gain access to an archaeological site that I submitted to the DG office of the ASI in New Delhi, I introduced my project as a "history of Indus archaeology" – and this had a desired positive effect. Nevertheless, with excruciating delays that often necessitated extra-bureaucratic interventions – most persuasively the office of the Director General of the AIIS[29]. But it was not always successful, and at least in two cases, I was denied permission to stay at the site and work. Legally as an Indian citizen, I have the full right to visit any archaeological excavation

[29] At one instance, an SA of an Ex. Br. out rightly lied to me that his Ex. Br. was not conducting any excavations that year, whereas through my informants I was aware that his branch was conducting a full-fledged excavation as I was speaking to him. Infuriated, I had to seek the help of the DG of the AIIS, who personally spoke to the SA, and asked me to visit him again. The second time when I met the SA, he immediately apologized to me and sheepishly remarked why I had not informed him earlier that I was recommended by the AIIS.

anywhere in the country. However, the permission I sought was to allow me to stay in an ASI camp, only possible through the largesse of the organization. For my ethnography, it was essential to stay and live in an ASI excavation camp, as it was important not just to observe the happenings in an archaeological site but also the social life in the ASI camp. Although I visited all the sites where the ASI Ex. Br. conducted excavations, I was only allowed permission to stay and work in four archaeological sites. I spent two-excavation seasons conducting ethnographic work in these ASI sites. The first year, 2003-2004, I worked at the sites of Dholavira, Baror, and Hasi and visited the sites of Juni Kuran, Chak 86 and Tarkhanwala Dhera. The second excavation season of 2004-2005, I worked at the sites of Baror, Bhirdana, and Hansi. The time I spent at the sites ranged from two and a half months to three weeks, depending on the period I was allowed to stay. The permission from the DG office did not specify how long I could stay at the site. That depended on the camp in-charge and the relationship that I developed with him. In most cases, I stayed as long as I desired.

The ASI archaeological site was a hierarchical professional organization and in order to gain the confidence of all the members of the organization, I had to explain my objectives to everyone I came in contact with. The level of suspicion and misgivings were high, and there were reasons. 2003 was an extraordinary year in the postcolonial history of ASI. It had recently conducted probably its most important excavation in the past fifty years - the Ayodhya excavations in March-August of the same year. It was a time of astonishing duress and pressure for the ASI archaeologists and staff as they had conducted the excavation under strict judiciary vigil and supervision. The daily workings of the ASI team had been under sharp public scrutiny. The four month long excavation was conducted under the daily surveillance of the national media and the judiciary. The discoveries of each day and the practice of ASI archaeology at Ayodhya made national headlines. The numerous groups involved in the Babri Masjid-Ram Janambhoomi case would level charges and counter charges against the ASI on a daily basis. This led to unexpected stress on the ASI archaeologists and the staff who were working at the site, and who were considered neutral and objective agents of the state – any of their pronouncements became 'sound-bytes' for the media. The court, overseeing the excavation, eventually barred members of the ASI to speak to the media.

The apprehension was further heightened because Central Bureau of Investigation (CBI) - the national criminal-investigating agency was scrutinizing corruption charges against a number of top ASI officials. In 2002, it had framed corruption charges against the ASI staff of the high profile Rakhigarhi excavation – the largest Harappan site in the country.[30] The shadows of both these events underlay my interaction with the ASI staff. An informant once told me after a few weeks of my stay at Dholavira that initially some staff members had thought that I was an undercover CBI agent investigating corruption in the ASI. Usually, within a week of my interaction with the members of the ASI staff in a site, most would come to know that I was an Indian citizen working as a historian and anthropologist and was affiliated to an American University. However, due to a heightened sense of suspicion, no one was willing to be interviewed on tape and soon I realized that if I was seen with my field-pad and pen, my informants would feel uncomfortable. For those who were involved in the Ayodhya excavation, a court order restricting them to divulge information was curtailing them, others were afraid to be pin-pointed. So all the ethnographic data used in this dissertation is based on my field notes that I would write in my tent in the camp twice a day - during the afternoon lunch break and at night before going to bed. The extensive quotes that I have used in the dissertation were therefore consigned to memory and written usually two or three hours after interactions with my informants. They should not be viewed as verbatim utterances of my informants. Furthermore, in such a circumstances, it becomes imperative for me to protect the identity of my informants. I do not use any names in this dissertation, I only use the official designations of my informants – Assistant Archaeologist (AA); Assistant Superintending Archaeologist (ASA); Deputy Superintending Archaeologist (DySA); Superintending Archaeologist (SA); Joint Director General (JtDG) and the Director General (DG).

Structure of the Dissertation

The dissertation is divided into two distinct parts. The first part, consisting of two chapters (2 and 3), is an ethnographic contemplation on postcolonial institutional governmentality. It concentrates on the social universe of the ASI as a postcolonial organization – the professional subjectivity of archaeologists, organizational hierarchy, and its interaction with the other. The second part with three chapters (4,5 and 6) is ethnography of postcolonial scientific archaeology. This part focuses on the epistemological practice of ASI archaeology – its daily

[30] Rakhigarhi, located in Hissar district of Haryana is the fourth largest of the known Harappan site. The ASI carried out excavations 1997-98 & 2000-01.

practice of discovery and knowledge production. The divisions are based on the theoretical emphasis of this dissertation as have been discussed in the earlier part of this introduction. Through an ethnographic account, the first part attempts at delineating the organizational structure of the ASI- as a statist institution involved in governance of both its own professional cadre and the archaeological spatiality it constructs in the fringes of the state. The premise for such a division is the logic that the epistemological practices of scientists are not disassociated with their social life – a reasoning that is based on the disciplinarian conclusion that research in sociology of science have revealed. The social and epistemological universe is intertwined in a complex fashion and, as I demonstrate in this dissertation, there is a direct impact of the social and organizational makeup of the ASI on its knowledge production processes. Here, I provide a short description of each chapter and the theoretical framework that informs the ethnographic account provided.

The second chapter, *Organization and Hierarchy in the ASI*, is a critical examination of the nature of organizational hierarchy of the ASI, and the impact of its structure on the lives of individual archaeologists who spend their whole career working for this organization. The focus of this chapter is on examining how professional subjectivity is formed while working in a highly hierarchical organizational structure, typical of postcolonial governmental institutions in India. I show how institutional and disciplinary conditions that are local and contingent shape the professional subjectivity of ASI officers as archaeologists. Drawing on Foucault's idea of discipline, I show that that the ASI is first a bureaucratic organization of the state and then an institution for knowledge production and argue that the highly codified and hierarchically structured excavation processes reveal the link between the sociology, epistemology, and ideology of the excavation.

The third chapter, *Spatial formation of the Archaeological Field*, critically examines the spatial formation of the archaeological field. The focus of this chapter is only on the first intervention of an excavation project - that of selecting the physical location of the excavation site and the setting of the archaeological camp. Through ethnographic description, it establishes the ASI archaeological excavation as a "(post)colonial exploration project" – a genre of colonial science which emphasizes that the "real" process of knowledge production is situated outside the domains of the metropole at the fringes of the nation. It investigates how the ASI, simultaneously functioning as a statist organization and as an archaeological

organization, transforms a landscape designated as archaeologically potent into a space conducive for the production of knowledge. This chapter destabilizes the meaning of archaeological fieldwork defined as an epistemological sphere and demonstrates that archaeological fieldwork is essentially an ideological engagement with landscape and a manipulation of the landscape's spatiality to assemble an epistemic domain conducive for the production of archaeological knowledge.

In the fourth chapter, *Epistemological Formation of the Excavation Site*, I show the ideological process through which an unexcavated archaeological site is transformed into an epistemic site that is made fit to produce objective knowledge. I specifically illustrate how a wild landscape located in the middle of rural India was transformed into a materialized epistemic space, an ideologically-laden abstract spatiality suitable for archaeological excavation. I demonstrate how an unknown space, which was designated as an archaeologically potent site, was prepared and re-configured into the iconic epistemic location – the excavation site.

The fifth chapter, *Acts of Discovery*, localizes my ethnographic intervention within the confines of the trench. I show how within the socio-political and the epistemological spatiality of the excavation site, archaeological artifacts are discovered and transformed into archaeological knowledge. I argue that the systematic archaeological process in the micro-context of the trench that produces scientific evidence for the construction of the narrative of past, is itself a convoluted practice.

The sixth chapter, *Politics of Representation*, focuses on key post excavational practices and processes of the ASI archaeological intervention at the site. Along with the epistemological operative of excavation, the ASI archaeologists actively engaged in the performative imperative of presenting and (re)presenting the evidence unearthed. I argue in this chapter that the ritual of the state and its fetish for superficial perfection subsumes these daily practices of the archaeology. The seventh chapter is the conclusion to this dissertation.

Chapter 2

Organization and Hierarchy in the ASI

Introduction

A senior Asst. Archaeologist (AA) in Baror, who was awaiting promotion, gave me a fascinating analogy. It tellingly revealed the significance of organizational hierarchy in the ASI. This was the first day of my second season (2004-05) in Baror. It was around 8.30 in the morning; I had just had my breakfast of fresh *parathas* and a glass of hot milk with other archaeologists and the excavation staff. The sun was shining, and the early morning fog had disappeared. It was a short walk from the camp to the site - from one barricaded space to another. As I started climbing the excavated mound, I could see that the floor of the trenches was still damp and the outline of the famous mud brick walls of Harappan architecture, common in this part of the Indus civilization, clearly visible. The AA was a middle-aged man with a rough beard, sporting a white baseball cap, a black woolen muffler covering his neck, a full-sleeved blue checked shirt, dark blue rugged jeans, wearing white Nike sport shoes (a replica bought from Delhi's Palika Bazzar) and fake steel-rimmed black Rayban sunglasses tucked in his shirt's breast pocket.

The previous season, when I had met him, he had seemed very confident and had exhibited a clear sense of control over the excavation site. However, this year he seemed a bit uncertain and as I gradually realized, depressed. When I had first come to the site in 2004, he had applied for the intra-ASI promotion interview for the post of DySA (Deputy Superintendent Archaeologist). While talking to him then, it had appeared that he was very sure of getting it; he had the requisite qualifications - experience, academic degree, and also the necessary "connections" among the "higher ups" in the ASI. The "interview went very well," he informed me as he was giving me a tour of the excavated area. “But luck was not with me," he said, showing me the newest discovery of the season and explaining to me the objectives of the current season's excavation. He believed that he should have gotten the job, but internal "favoritism and politics," along with "reservations" for the lower castes, had negated his chance. For the last thirteen years he had been waiting for a promotion. He now thought that he might have to wait few more years before he got any promotion. The prospect of being an AA for the next five to seven years seemed daunting to him. He remarked, disheartened, "I am staring at a stagnant future, and more than half my career will be over."

He had been shocked to discover that the colleagues who had been promoted, and believed that they were not capable of becoming DySA. He and some other AAs had filed a writ petition in the Delhi High Court against the promotions, which they argued were rigged. They had been successful in getting a stay order on the promotions, but the final hearing was due in the next few weeks[31]. It was in this context that our discussion turned towards the organizational hierarchy of the ASI and the way he perceived his status as an AA.

The AA had joined ASI in the early nineties, and had been an AA for nearly twelve years. He was the last of the AAs from his cohort awaiting promotions. My perception of him was that he was very upset and frustrated with the ASI (like a lot of the informants that I interacted with during my fieldwork). He had been fascinated with archaeology in high school and had gone on to do an MA in Ancient Indian History and then had been admitted to the Institute of Archaeology, to do a Diploma in Archaeology. After the diploma he had been unemployed for a couple of years and had worked as a "daily wage" archaeologist in a couple of ASI excavations of historical sites in north India. Then in 1993, he had been recruited as an Asst. Archaeologist. He had applied for a number of internal promotions but had not been successful, and so his depression and frustration had grown manifold. He said he was "resigned to his fate, but not without a fight".

We were walking across the excavated areas, balancing ourselves on the balks of the Wheelerian grid, careful not to let dirt fall into the trenches, some of which were two or three meters deep. The trenches were mostly empty, without any structural features, and had four or five local men and women (laborers), working, huddled together in a circle, crouched with their brushes and knives, slowly removing dirt from the floor of the trench. Neither of us wanted the dirt to fall on their bodies, nor were we keen on losing our balance and falling into the trench. As the AA was talking about his trajectory in the ASI, half in jest, half seriously, he offered an analogy for hierarchy in the ASI with the *varna* system in the *Rig Veda*. He clarified that this was not his *theory* but he had inherited it from a senior, retired ASI archaeologist Dr. Srivastava, who after spending an entire career in the ASI, retired as a DySA.

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[31] After a couple of months I came to know that the petition was rejected and the promotion of AAs who had been promoted to DySA was confirmed.

He said that Dr. Srivastava had often compared the hierarchy in the ASI to the division proposed in the *Purusha Sukta* in the tenth *Mandala* of the *Rig Veda* (10.90)[32], which describes the ritual sacrifice of the primordial man. This hymn has become the quintessential text from the Rig Veda, often invoked in popular culture in contemporary India to explain the mythological foundation of the caste system in India:

> "The Brahman was his mouth, of both his arms was the Rajanya made.
> His thighs became the Vaisya, from his feet the Sudra was produced" (Griffith 1920).[33]

The hymn is often interpreted as the cosmic manifestation of the caste system in ancient India (see: Dumont 1980; Srinivas 1987; Quigely 1994; Smith 1994; Olivelle 1999; Gupta 2000). During the ritual sacrifice of the primordial man, the highest caste- *Brahman* emerged from the mouth of the primordial man, the *Kshatriya* or the warrior caste emerged from the arms; the *Vasiya*, the trading caste from the thighs; the *Shudra*, the lowest caste emerged from the feet. The *Pusrusha Sukta* is one of the most significant hymn of the *Rig Veda* which has been employed by colonial scholars and officials since the early nineteenth century to explain the hierarchy in Indian society and it has had powerful valence in contemporary postcolonial India. This cosmogenic metaphor has since been incorporated in the popular culture of middle class Indians especially after caste based affirmative action was legalized in the Indian Constitution for the lowest castes (Scheduled Castes and the Scheduled Tribes) in 1950. Furthermore in 1989, there was a 23% increase in caste based reservations, on the basis of the Mandal Commission Report, which recommended that, not just the Scheduled Caste and the Scheduled Tribes but also the Other Backward Castes (OBCs) be provided with reservations in government jobs, and academic institutions. This led to widespread violence in urban India (see: Rao 1990; Engineer 1991; Roy Burman 1992; Maheshwari 1995).[34] Extensive media

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[32] The hymn is repeated in the *Atharvaveda* (19.6), the *Samaveda* (6.4), the *Yajurveda* (VS 31.1-6), the *Taittiriya Aranyaka* (3.12,13), and it is commented upon in the *Shatapatha Brahmana*, the *Taittiriya Brahmana*, the *Shvetashvatara Upanishad* and the *Mudgala Upanishad.*

[33] "*Brahmano asya mukhamasid bahu rajanyah kritah |*
Urutadasya yad vaishyah padbhyam shudro ajayata ||" (Rig Veda 10.90.12)

[34] As I write this chapter (April-May 2006) another bout of unrest has erupted over caste-based reservations in urban India. Students from elite central government institutions, specifically from the IITs (Indian Institute of Technology), IIMs (Indian Institute of Management), AIIMS (All Indian Institute of Medical Sciences) have started a mass movement and protest against fresh reservations for the OBCs, which were announced by the Union Human Resource Development Minister, Arjun Singh at the end of April. Most of these students come from the upper echelons of the caste system and they feel threatened by the decreasing space for them in educational institutes. The growing middle class in

coverage and heated public debate brought hymns like the *Pursusha Sukta* and other Sanskrit liturgical texts, which sanctified the caste system, into the mainstream of popular culture in India. It was not a surprise to see an upper caste Brahmin Asst. Archeologist using the *Purusha Sukta* analogy. He had been "severely hit by the reservation system" in the ASI, and he claimed that, like thousands of students, he had protested against the Mandal Commission Report in Bareilly, his hometown, during the student unrest in the fall of 1989. He started off by saying: "like in the *Rig Veda*, in the ASI a strong hierarchy exists." As he went on, I assumed that he would complain about the reservation system in the governmental jobs, which was a very heated topic of discussion in the ASI, especially when recruitment and promotions were being conducted. "The *Brahmins* are those *babus* (bureaucrats) who sit in the D.G. office, the senior officers, who chart out major plans, sit in planning meetings, and give general directions to the ASI. The *Kshatriyas* are the archaeologists who work in the Excavation Branches - especially the SAs and the Dy.SA, who work in the field - the battlefield. Whenever the ASI is challenged by outsiders about it's contribution to the world, it is the work of the archaeologists in the field that is showcased - they are the frontline soldiers of the ASI. The *Vaisyas* are those officers who work in the Circle Offices, especially the Conservation Assts. who make a lot of money. They are the real traders and businessmen. The *Shudras* are workers like us, the AAs, the technical staff, the lowest in the hierarchy, the draughtsman, the surveyors, and the artists. We are the ones who work the hardest [*hum sabse jyada khatathe hai*]."

As he completed his analogy, the AA suddenly jumped off the trench balk we were walking on, into the trench, about a meter deep. He went towards a middle-aged laborer, with a grey moustache supporting a week long stubble on his rugged face, wearing a printed cotton bush shirt and a pair of brown *terrycot* trousers, a white turban covering his head and half his face, who was kneeling barefoot in the trench. The laborer had a "section cutter" (a wood and iron axe like contraption with a very thin blade) in his hand, and he was concentrating on straightening the wall of the trench - the stratigraphic section - by scraping the uneven portion of soil jutting out. "What kind of a section cutter are you? You have been working at the site for two years and you still cannot do a simple task of cutting a section straight?" Yelled the AA to the laborer. The laborer got up and looked at the AA sheepishly as the AA asked him to

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India and a stagnant higher education system unable to cope with the growing population of aspiring students have aggravated the problem.

come up on the top of the balk and rebuked him again. He gestured to the laborer to look carefully from the top of the trench balk, and see how he was scraping *into the* trench wall rather than being straight. After a few instructions, which seemed more like admonishments, the AA was back up on the trench balk where I was standing and we continued the site tour as if nothing had happened. He then looked at me and nonchalantly remarked, "one has to frequently reprimand the laborers or it is impossible to get work out of them" [*laborer ko hardkana jaruri hai, nahi toh en se kaam nikalna mushkil hai*].

The Hindi word *hardkana* was north Indian slang. It literally meant shaking or vibrating. This was a condescending term and was usually used in the context of disciplining a subordinate working class subject: household servants, farm hand, or daily laborer. The term even had its place in the domestic hierarchy, being often used to describe the use of control over wives and children. I had heard this term being used a number of times in the ASI. Its application was always in the context of the laborers and act of disciplining them. The expression *hardkana* can be viewed as an operational term that was a metaphor for the relationship between senior and junior members of the ASI hierarchy. I heard the word being used by AAs and DySAs in the context of disciplining their students and the technical and non-technical staff members of the excavation team. This term was also used in a similar context by SAs when talking about disciplining junior officers, staff member, students, and laborers.

In fact, the AA's analogy was much more striking than what he described. Similar to the *Rig Vedic* hymn, his description of the ASI hierarchy failed to mention the *Ati-Shudras* or the untouchables. He had unconsciously erased their presence in his narrative. In the *Pursusha Sukta*, the lowest in the caste hierarchy were the *Shudras* who emerged from the feet of the primordial man, but this group was still subsumed under the dominant caste hierarchy. However, the *Pursusha Sukta* does not mention, and erases the presence of a large group of people who the *Rig Vedic* Aryans were in constant struggle with. These were the ***Dasyu,*** who in later Vedic and post-Vedic texts were described as the *Ati–Shudras*. They were the untouchables, who were outside the caste system of the Rig Vedic community. They were the perpetual outsiders, always subjugated in the mythic battles of the Aryans. Likewise, the AA failed to mention those that were outside the ASI hierarchy - the laborers. In all the excavation sites that I worked on, they were the largest category of people employed at a site. They were temporary workers numbering from more than a hundred at small sites like *Tarkenwala Dera*

to around five hundred at sites like *Dholavira*. They were also outsiders in the ASI hierarchy. They worked on the minimum daily wages that the local revenue department had stipulated and were non-unionized temporary workers who worked in the trenches, excavated the soil, removed it, and did the most labor intensive work at the site. Some, who were more fortunate, worked as *chowkidars* (watchmen) and servants[35] in the excavation camp, cooking, washing clothes, shaving and cutting hair, and even pressing the tired feet of the ASI officers and staff members. The irony of the AA's analogy was that he had structured a narrative of self-pity. He had framed his subjectivity as the lowest in the ASI hierarchy, which was indeed true. But in this narrative of self-pity, he failed to mention the lowermost populace and the most significant member of a postcolonial archaeological excavation - the laborers.

Professional Subjectivity and ASI

This chapter is a critical examination of the nature of organizational hierarchy of the ASI, and the impact of its structure on the lives of individual archaeologists who spend their whole career working for this organization. In this chapter, I will concern myself with the first four *varnas* of the ASI and will discuss the role of the laborer in the ASI's archaeological practice in the next chapter. The focus of this chapter is to investigate how professional subjectivity is formed while working in a highly hierarchical organizational structure, typical of postcolonial governmental institutions in India. Employing ethnographic examples as empirical evidence, supporting it with archival and textual evidence, I attempt to show how institutional and disciplinary conditions that are local and contingent, shape the professional subjectivity of ASI officers as archaeologists.

Theoretically, this chapter is a Foucaldian analysis of the ASI's organizational discourse investigating the formation and perpetuation of professional subjectivity. I borrow the analytical apparatus for this examination from organizational studies, where Foucault has been employed to interrogate the role of organizational discourse and subjectivity (Knights & Collinson, 1987; Burrell, 1988; Cooper & Burrell, 1988; Miller & Rose, 1988; Rose, 1989; Townley, 1993, 1994; McKinlay & Starkey, 1998). For Foucault, subjectivity relates to the circumstance of being subjected to, or an object of, power through power/knowledge relations (Foucault, 1980, 1982). Individuals are then transformed into subjects that secure a sense of

[35] Although the Hindi word is *naukar* for servants, it was never used to describe these laborers who worked on the camp site rather than the excavation site. This term was considered derogatory. They were also called *chowidars*, even though the tasks they did were primarily domestic chores.

their own meaning, purpose, and reality through participating in the discursive practices that are a condition and consequence of power/knowledge relations (Knights & Morgan, 1991). For Foucault, the subject 'is not the speaking consciousness, not the author of the formulation, but a position that might be filled in certain conditions by various individuals' (1977: 115). This analysis argues for a radical grounding in ontological constructivism in which reality is literally 'talked and texted' into existence. It asserts that there is nothing outside discourse but more discourse; all reality, natural and social alike, is discursively contingent and fabricated. This form of Foucauldian analysis has been criticized as tending to idealize meaning and "to marginalize the non-semantic aspects of economic and political reality in that it is ontologically insensitive to material structuring and its constraining influence on social action" (Reed 2000: 524). However, I still find it useful in utilizing this framework to analyze the role of institutional hierarchy in the making of professional subjectivity in the ASI. The ethnographic experience that I accumulated over the course of my fieldwork explicitly shows that the ASI as an organizational entity has a tremendous impact on the professional subjectivity of the individual employee, from the archaeologist to the laborer working in the excavation. The discursive ideology of the ASI does not allow any movement beyond the social and organizational bounds that it has created. The individual actors are almost completely stripped of their agency and are subjected to enormous control by the organizational discourse of ASI.

In the various studies referred to above, organizational discourse has been defined as "instances of talk, text, and conversations that take place within organizational 'boundaries'" (Bergstrom & Knights 2006: 355) and is substantiated by detailed analysis of written text and spoken interaction (Fairclough 2005). Robert Chia has underscored the importance of organizational discourse in order to comprehend the subjectivity of individual while working in an institution:

> The study of organizational discourse, and the way it shapes our habits of thought, by legitimizing particular objects of knowledge and influencing our epistemological preferences, is crucial for a deeper appreciation of the underlying motivational forces shaping the decisional priorities of both organizational theorists and practitioners alike. For, by organizing our preferred modes of thought, organizational discourse works as a relatively unconscious force to restrict vision and to thereby inhibit the exploration of genuinely alternative modes of conception and action (Chia 2000: 514).

The analysis in this chapter has been influenced by the above definition of organizational discourse.

During my fieldwork I observed that the ASI organizational hierarchy had a profound and long lasting influence of the lives of the people who worked within it and it also affected the process of knowledge production they were involved in. I realized that the officers in the ASI were first employees of the state and only then archaeologists. The disciplinarian apparatus of the postcolonial state permeated the daily practice of each member of the archaeological team; especially in the form that organizational discourse was articulated in the camp and at the excavation site. The ASI excavation team was not a mere archaeological organization that had pitched its camp in the countryside to excavate the dead remains of an ancient civilization, but it was an ideological apparatus of the state which mediated the forms of knowledge it produced and reproduced, and which also had an enormous impact of the subjectivity of an individual knowledge producer.

Organizational discourse was articulated in the ASI through various statist practices, which included inscriptive texts like memos and orders, but in the main, it was articulated through interpersonal interaction in the offices and the excavation sites. As an ethnographer, I had little access to either of these instruments of articulation. However it was through conversations and informal interviews with my informants and participation in group-activities that I accumulated most of my evidence for this chapter. It was during these long conversations like the one I have described above -- in the camp, during breaks, or at night while relaxing -- that I was able to hear narratives, stories, anecdotes, gossips, and rumors that made up the social life of ASI staff members. The ASI was the single most important thing that defined their social and public life; most officers and staff I spoke to differentiated between "office life" [*sarkari*] and "private life" [*parivarik*, often the English words 'family' or 'private' were used]. Life at the excavation was a constant engagement with the disciplinarian and the organizational formation of the ASI. The distinction between office life and private life was blurred during excavation. The excavation period led to the formation of intimate circles of friends that virtually lasted a lifetime. For example, in most sites, the officers would hang out among their own kind, the technical staff would huddle together in their own tents, while the students would forms their own groups. I would spend as much time as possible during my stay at each camp with all the group members talking to them. It was during these times of

camaraderie that informants would open up and talk about their life and work. This interaction formed an important part of the daily lives of all the members of an excavation camp; this "relaxation" period was also the time for discussing the highlights of the day, not so much concerning the nature of artifacts and structures discovered, but concerning events and occurrences of the day. The majority of these conversations were in group settings where several staff members would join in with their own stories, past narratives, and anecdotes. The ethnographic evidence provided in this chapter is based on these conversations and stories that I heard and assimilated.

The organizational State of ASI

The ASI, with its history of nearly 150 years, was a massive archaeological organization employing more than a thousand personnel as its permanent workforce and a couple of thousand temporary daily wage workers, including laborers who were involved with the conservation and excavation work. The structure of this organization has not remained constant, but evolved over the years. However the general framework of the organization has remained almost the same for more than a century. Historically, organizational changes were effected on the basis of various internal assessments and reviews such as the Wolley Review Committee 1939, Wheeler Review Committee 1972, Mirdha Review Committee 1984[36]. These committees were formed at historic junctures when general stagnation was perceived, and the ASI was going through a time of crisis. Structural reforms were undertaken on the basis of recommendations from these committees and sometime the changes would be effected nearly a decade after the recommendations were submitted.

During my fieldwork, I came to know that as recently as 2001, another review committee had been set up, called the *Review Committee on the Functioning of the Archaeological Survey of India*, also known as the Lal Review Committee 2001, in common parlance. It had the express goal of understanding the reason behind the "moribund state of the Archaeological Survey of India, comparing this state with what it had been like in the past, and to look for suitable remedial measures needed to put the ASI back on the rails" (Lal 2001: 25).[37] The observations

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[36] For commentary on the impact and effect of the Wolley Review committee see Possehl 1993 ; Chakrabarti 1988: 174.

[37] The terms of Reference of this Review Committee were:

of this committee were very critical and "hard hitting" as one SA put it, and therefore the "Ministry was sitting over the report and not interested in doing much with the recommendations". So by the time I arrived to do my fieldwork, according to the report, the "decline" (Lal 2001: 29) of this "great archaeological organization" (Lal 2001: 30) had already set in. The authors of the report argued: "For the past decade or so, there has been a steep fall in the performance of the ASI, whether it relates to exploration or excavation or conservation or research or publication" (Lal 2001: 29-30). Although the report had officially been submitted, it had not been made public or even published for internal circulation. Almost all the ASI officers I spoke to were aware of this report and its chief recommendations. However, very few had had access to it and had read it. Within the ASI circles, this report had an aura of a secret document and even I had to go from pillar to post to finally get access to it.

The organizational structure of the ASI during this "moment of decline" was both intricate and large-scale. The organization was headed by a Director General (DG), and the major task of the organization was separated in three major categories - exploration and excavation work, conservation work, and protection and preservation of heritage and archaeological sites. During my ethnography, I observed that the ASI employees, in their daily practice, engaged with the bureaucratic structure of the ASI mediated by three levels of power structure, defined spatially - the DG Headquarters Office, the Circle Offices, and the Excavation Branches (which existed both as an 'office' and a 'field'). The career of an officer in the ASI would predominantly shuttle through these three official locations. There were multiple other official sites that could be occupied by ASI officers; these were usually not considered desirable but rather an inevitability of a "government job."

The career of an archaeologist in the ASI usually began as a student at the Institute of Archaeology. With the right "luck, caste, and connections" [*kismet, caste aur connection*], the student, after a Diploma from the Institute, joined the ASI as a junior officer – an Assistant Archaeologist (AA) – through a written exam and an interview conducted by the Service Selection Commission (SSB). With the "luck, caste and connections configuration remaining

"(i) Restructuring of the Archaeological Survey of India organization and other things so as to make it more professional and effective; (ii) Restructuring of the Institute of Archaeology; (iii) Reviewing the recruitment Rules of Group A officers of the Survey and making suitable recommendations; (iv) Recommending ways and means to improve the working of Technical and Academic activities" (Lal 2001: 2). The report was submitted in installments between 200 and 2001.

constant," in the words of one informant, the AA would be usually promoted to Assistant Superintendent Archaeologist (ASA), and then to the post of Deputy Superintendent Archaeologist (DySA) and then a Superintendent Archaeologist (SA) before he retired from the "service".

The second rung of the hierarchy consisted of the SA, who would head the Circle offices and the Branches. I was told that there were more or less an equal number of SA and DySA's in the ASI. Therefore, depending on the seniority and cadre, a DySA could be promoted to the post of a Superintendent Archaeologist (SA), and subsequently to the post of a Director. But there were only about a dozen or so Directors in the ASI, and the promotion from SA to Director was solely on the basis of merit. "(M)erit basically means a lot of politics and networking" remarked my informant. The Director's post fell under the highest posts in the ASI hierarchy -- two Joint Director Generals (JtDG), an Additional Director General (ADG), and finally the Director General (DG). However, for more than a decade, these posts had been outside the reach of the junior ASI officers. Not a single Director had been promoted to the post of the JtDG, ADG, or the DG since 1989. This problem, the Lal Committee argued, was the cause of the decline in the ASI. They noted: "In the absence of any prospects of promotion in the foreseeable future, it would be too much to expect that they would continue to give their best to the ASI. Such a moribund situation has resulted in rapid deterioration in the quality as well as the quantum of the output" (Lal 2001: 30).

Each of these promotions could take as less as five years or as much as ten years depending on how the promotion "files moved in the DG office". At times an entire career would go by and an officer would retire without a promotion. I heard multiple stories about AAs, ASAs, and DySAs, who, because of multiple reasons, had been stuck in their positions for as long as ten years. One of these "horror stories", poignantly appears in a letter written by B.B. Lal as the Chairman of the Lal Committee, to the Minster of Human Resource Development, under which the ASI comes. Written in the March of 2001, Lal angrily notes:

> "The Committee visited Kalibangan to determine the conservation measures to be taken to preserve for posterity this unique site of the Harappan Civilization. During the visit the Committee met Shri Sant Lal, Deputy Superintending Archaeological Engineer, Jaipur Circle. The committee was shocked to learn that this officer, recruited through UPSC in 1978, has not got even a single promotion all these 23

years, even though higher vacancies were available and Shri Lal's record has been good. The same has been the fate of the other officers recruited after him. What a picture of utter indifference!!" (Lal 2001: 22).

This was not an isolated case. During my fieldwork, I often heard such narratives and even met a few such officers who were frustrated and depressed looking at their whole career pass in front of their eyes without adequate promotions, credit, or recognition due to them by law. These senior officers I met must have been keen archaeologists, but, now, they were not engaged with their work and had no higher aspirations other than spending their time in ASI and retiring. As an ASA who had joined the ASI, more than 25 years ago, and been promoted only once, told a young AA during a conversation about career prospects in the ASI: "we just want to pass out time in the ASI. Now you all should work" [*Hume toh ASI mein abhi time katna hai. Ab aap log kaam kare*].

I was informed, that on the basis of the recommendations of various committees, over the course of the years, "outsiders" had been inducted into the ASI at the level of the DySA and SA, in order to fulfill the shortfall of officers. This had been done through interviews conducted by the UPSC, in which internal candidates, ie. AA, ASA, or DySA, who fulfilled the determined criteria could also apply along with applicants who were not part of the ASI. The outside candidates included university lecturers, unemployed archaeologists with PhDs, museum curators, or archaeologists from the State Directorate of Archaeology. According to all the officers I spoke to, this incursion of non-ASI "outsiders" into the ASI had caused severe problems. "In some cases, young AAs were promoted to the rank of DySA or SA. They suddenly became the bosses of officers who were of a senior cadre, causing resentment. Further, in cases where an outsider was taken in, the promotion channel for junior officers was sealed forever," explained an ASA. The problem had become so acute that the organizational structure of junior officer cadre reflected an hourglass. At the base were a large number of AAs and at the top were a large number of DySAs whereas in the middle, there were very few ASAs. Many AAs that I spoke to were very anxious about this state of affairs and admitted this caused perpetual "tension" in their lives and work, as very bleak career prospects were "staring in our face". Taking note of this organizational structure, the Lal Committee reports: "because of the faulty cadre structure there has been acute frustration or stifling stagnation sapping the cadre of all energy" (Lal 2001: 37). They argue that the crux of the problem has been the absence of the mandatory cadre review once in five years: "not a single cadre- review

has so far been done for the ASI. But, most other departments of the Government of India have benefited, repeatedly, because the Administration there had conscience and some accountability and organized cadre reviews" (Lal 2001: 39). The Lal committee placed the blame on the top-most in the hierarchy of the ASI (the DGs), who were not archaeologists but bureaucrats from the Indian Administrative Service (IAS) on deputation in the ASI. They accused these deputed IAS DGs for the organizational chaos:

> "Such agony may not be seen anywhere else in the Government of India. Still, this state of affairs has not pricked the conscience of any of the deputationist Administrators entrusted with the Cadre management of the ASI for the last two decades. Their achievements have not been just zero, but stunningly negative, destroying the careers of about three generations of officers and staff when we reckon that unless the person climbed the ladder, step by step, during the first ten years of service, in unison, they are stumped or stampeded out. The deputationist had no stakes in the ASI and no accountability to anybody in the ASI. Their careers were secure in their present cadres without vicissitudes which could strike them had they been there. They got promotions, even accelerated promotions, in their cadres. The ASI had been a "rest-house for them" (Lal 2001: 40).[38]

I conducted my fieldwork in the background of this kind of utter resignation and discontent that prevailed through the rank and file of the ASI staff. I observed that the organizational structure of the ASI was configured in a way that exacerbated institutional oppression. Each rank member blamed his higher officers in the hierarchy for systemic oppression, in the form of arbitrary transfers, delayed promotions, exasperating work conditions, or inadequate infrastructure to produce "quality work." This was a cause for constant tension and high stress levels amongst all cadres of officers. However, each cadre conveniently erased its own complicity in this systemic oppression of the statist institutional apparatus. The Report of the Lal Committee, which consisted of two retired DGs of the ASI, other officials of the Government of India, and members of the archaeological community, was an extraordinary example of such non-reflexive subjectivity. The report did not even make a mention of the state of the laborers in the excavation sites and camps, but a great deal of space was devoted to

[38] They also note: "the top posts at the top three levels in the Department have been kept vacant or filled by deputationists choking the breath of the cadre...It appears, in the ASI, the fence has been eating the crop to be guarded" (Lal 2001: 55).

the chaos abounding in the top most echelons of the ASI. The committee had very little to speak about the role of the same archaeologist officers (SAs, Jt.DG, ADG) in producing and reproducing the stifling conditions that plagued the ASI. In the pages below, I show how the systemic repression within the ASI was not just a product of non- archaeologist DGs, but more a result of the failure of the postcolonial institution in comprehending the systemic oppression it exerted on its own employees. I argue in this chapter that the institutional apparatus of the postcolonial state did not distinguish between the subjects it employed and the subjects it governed. The oppressive impact of its institution was all-pervasive and its influence was also exerted on the members that constituted its structural apparatus.

The Headquarters

Organizationally, at the top of the ASI hierarchy was the DG, whose office was located in the national bureaucratic district in New Delhi, perched between the National Museum, housing the treasures of Indian art and antiquities from the Harappan period, and the Vigyan Bhavan - a premier conference hall of the government of India, the venue for some of the most important national and international conventions and conferences held in India. The DG office was often referred to as just "Headquarters." This Headquarters was a large colonial bungalow of Lutyens Delhi,[39] which was not big enough to house the growing staff of the Headquarters.

Like typical governmental offices in India, it was overcrowded and cramped. The rooms were actually large bedrooms and living rooms converted into partitioned office spaces. Desks, tables, and chairs jostled with each other for space and competed with steel racks, wooden cupboards, and cabinets overflowing with files. Men and women could be seen crouching over their tables doing paper work, hidden behind stacks of files, or peering into antiquated desktop computers. The quivering sound of ceiling fans or the deep drone of air coolers would penetrate the dry heat of Delhi, when I visited the DG office. Most of the people I spoke to were not happy with the workspace given to them but neither did they want to move into larger premises. "This is just too centrally located," explained a staff member who had been working in the headquarters for nearly a decade. I was told that the Ministry had acquired land for a large office space in South Delhi, outside the bureaucratic district, but the ASI officials were reluctant to move because they did not want to leave this "prime location." In a moment

[39] Sir Edwin Lutyens (1869-1944) was a British architect who designed New Delhi, a project that was completed in 1929 and officially inaugurated in 1931 (see Irving 1981; Volwahsen 2002). This colonial landscape now houses the nation's Parliament, the President's house and all the offices of the Ministries of the Central Government.

of subtle observation, the Lal Committee attributes the cause of the chaotic state of the ASI to the abysmal state of headquarters. They sarcastically note:

> "The Headquarters' office is by and large buried in files, with the result that it has no time to monitor and carry out an on the spot supervision of what is being done or being neglected by the Circles, which number as many as nineteen, the five Excavation Branches, the Science and Epigraphy Branches which too have many sub-offices and so on" (Lal 2001:33).

This prime location appeared to give the ASI its status as one of the most significant survey organizations of the Indian state. The ASI employees claimed that this central location of the ASI headquarter made it the most valuable of all the survey organizations in India and bestowed on it a unique status. All other government of India survey organizations - the Geological Survey of India, the Survey of India, the Anthropological Survey of India - did not have their Headquarters in New Delhi[40]. Thus, by virtue of having its headquarters in New Delhi, the ASI got a special place in the bureaucratic structure of the government of India and became a truly "central government organization." This was important for all the informants I interacted with during my fieldwork. New Delhi was perceived not just to be the political centre of the country, but also the power center of the country. To be affiliated with an organization that was located in this power centre was considered a vital status symbol by all who were involved with the ASI. An Asst. Archaeologist in Dholavira, explaining the importance of being a *Dilliwale* (an inhabitant of Delhi) at the far away fringes of the nation in Kutch, remarked: "we do not get as much respect as military officials, but even the military respects us when they come to know we are from Delhi" [*Hame military jaisi ijat toh nahi milti hai per Dilliwale hone ke kaaran military wale bhi ijat karte hai*]. It was poignant that even temporary laborers who worked at the excavation site, far away from New Delhi, thought it was prestigious to be working for a central government agency with its headquarters in the *rajdhani* (capital city). This politics of location was important in the self-imagination of ASI employees, and instilled in them a sense of self-importance, which was essential to their daily working as government officials, far away from the centre of the nation.

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[40] The Geological Survey of India and the Anthropological Survey of India had their head office in Calcutta, whereas the Survey of India had its Head office in Dehra Dun, a plainland town at the edge of the Himalayas in north India.

The Headquarters housed the offices of the topmost officers of the ASI along with the office of the Director General. The offices of the Additional Director General (ADG), two Joint Director Generals (Jt. DG), and eleven Directors of various departments were also located there. These eleven departments spanned all the work of the ASI: Administration, Expeditions Abroad, Conservation, Mission and Project planning, Publication, Monument (two), Antiquities, Exploration and Excavation, Central Archaeological Library, Museum. These senior most officers of the ASI were supported by a number of Superintendent Archaeologists (SA) - some who had independent charge of some sections – and a host of Deputy Superintendent Archaeologists (DySA), Asst. Superintendent Archaeologists (ASA) and Assistant Archaeologists (AA). A retinue of technical staff, clerks, peons, and temporary workers formed the bulk of the whole organization at the headquarters. The DG office was at the top of the ASI "chain of command" and strictly controlled the daily workings of each and every aspect of ASI. It was not just symbolically the centre of the ASI but the central source of all power in the ASI.

Both the terms "Headquarters" and "Director General" were vestiges of the colonial military genealogy of the ASI, and signified in a powerful way the relationship between the DG office in New Delhi and the rest of the offices in various parts of the country. The organizational structure of the ASI was set in such a way that the Headquarter had hegemonic control over the working of each and every official offshoot of the ASI. Even in temporary excavation sites of the ASI, far away from the physical reaches of the Headquarters, predominantly under the control of the Excavation Director, small and minute daily decision were subjected to the DG office's authority. For example it was prohibited to take images (still photography, video, or moving images) of any ASI site while excavation work was in progress, until the ASI had published the report of the excavation. However there were exceptions to this rule and permission could be obtained, but that had to be granted only by the Director General; the Excavation Director did not have the authority to issue any such permission.

When I arrived in Dholavira in the December of 2004, I met a team of documentary filmmakers who were making a television episode on the Indus valley civilization for Discovery Channel. While talking to the director of the unit, I came to know that they had had a very difficult time obtaining permission to shoot at Dholavira. He said that that they had finally enlisted a public relations firm in New Delhi to lobby at the DG office. It had taken

them more than a month to get the permission to shoot at Dholavira and this had involved, among other things, sending a bouquet of roses to the DG every day, who at that time was a woman. It seemed that the filmmaker's PR firm had the desired effect; not only did they get access to the site, but the access was to such a great degree that the Excavation Director even complained to me about the team. The complaint was justified, since the team from Discovery Channel had even been successful, during their shoot, in uncovering the famous Indus Script signboard at Dholavira which, after its discovery, had been reburied and exposed to the elements only very rarely.

The Director General

The post of the Director General of the ASI was a very controversial and contentious one. Since the early 1990s, this post has been mired in controversy, and this has had a significant impact on the character of the ASI, and the self-image of archaeologists working there. Since the ASI's inception, and especially after independence, ASI archaeologists considered it to be a national organization whose sole purpose was to produce the "glorious heritage of ancient India and bring it to the attention of the nation". The ASI archaeologists believed that they were involved in a national task: "*hum bharatiiyatha ke liya kaam karte hain*" (we work for the nation). They believed that the knowledge produced by the ASI was critical because it allowed a postcolonial nation like India to be proud of its heritage and past. Central to their pride was the belief that the ASI was not a mere bureaucratic organization like a department in the Ministry of Irrigation and Water. "We are different because we are primarily scholars involved with the task of producing knowledge for the nation, housed in a bureaucracy," explained an Asst. Archaeologist, "but things have changed since the post of the DG and other top levels of ASI have been captured by the IAS (Indian Administrative Service) mafia". He continued "since the inception of the ASI in 1858, it has been led by an archaeologist, a practice that continued for more than a century. But in 1989, for the first time, a non-technical IAS officer was made the DG of the ASI. This has affected the morale and the workings of the ASI a great deal". The Asst. Archaeologist further explained, "IAS DGs don't know anything about archaeology, some of them are aware of it, but most cannot make out the difference between a Harappan and an early historic site. They don't understand that archaeology is a field science and that the ASI is not a standard governmental organization. The post of the DG

of the ASI is not considered to be prestigious among IAS circles and the only incentives that the DG gets are government-sponsored trips abroad showcasing Indian heritage. So they don't actually care about the real worth of the ASI." This was not an unusual sentiment. All the archaeologists I spoke to alleged that there had been a serious degradation in the workings of the ASI since the post of the DG had been taken over by IAS officers. One Excavation Director even went to the extent of telling me that "the death knell of the ASI was struck the day an IAS officer took over as the DG of the ASI."

I first came to know about this controversy in the December of 1994, when as an MA student of the Deccan College, I attended the infamous plenary session of the World Archaeology Conference (WAC 3) in New Delhi (see: Navalakha 1994; Muralidharan 1994; Golson 1995; Hasan 1995; Colley 1995; Sawday 1995). Among the many motions that were presented was one that concerned the restitution of the post of the DG from the IAS. It argued that the ASI was a "scientific and a technical" organization and from "the day it was established, the DG was always an archaeologist"; therefore it was necessary that the post be restored to the "technical person". This motion was presented by the ex-DGs of the ASI, along with some senior archaeologists of the ASI, and met with resounding approval, especially from the Indian delegates present in the Hall. Since then, this has been a controversial and much discussed issue in the rank and file of the hierarchy of the ASI. Almost all the informants I spoke to argued that the present deteriorated state of the ASI was because "we are without a true leader. There is no inspiring archaeologist like Wheeler or even like Dr. B.B. Lal leading us", explained a draughtsman at Bhirrana. "Things were different when the DG was an archaeologist. He would come to the excavation site and jump into the trench, scratch the trench floor with his knife or draw the stratigraphy lines on the trench wall. They knew what archaeology was all about. They were archaeologists. They lived like us in the tents, ate the food we cooked. Since they were archeologists, they were sensitive to our concerns, from the daily needs of an archaeological camp to issues related to postings, promotions, and transfers of officers".

Over the last fifteen years it is not just the post of the DG of the ASI that has been taken over by the IAS cadre, but other top posts of the ASI have also been allocated to the IAS. Thus, along with the post of the DG, the posts of Additional Director General (ADG) and the two Joint Director Generals (Jt. DG) were held by officers of the IAS. This clogging of posts had

caused a considerable amount of frustration for ASI officers, who joined the ASI as Asst. Archaeologists and spent their entire careers hoping to reach the top of the organization before they retired. Very often, the blame for all the ills of the ASI was placed not just on the non-technical DG, but to all the top officers of the ASI who belonged to the IAS. An Asst. Archaeologist remarked "how can they as non-archaeologists who have never been in the field, understand what fieldwork entails? They think fieldwork is like vacation, away from the drudgery of office work that they as bureaucrats are used to and hate. They envy us and therefore don't want to understand fieldwork". He was specifically talking about the then ADG, who was also an IAS officer and managed the Administration section of the ASI. "The DG of the ASI needs to be an archaeologist, who not only understands ancient Indian archaeology and history, but also knows what it means to work in the field. He should be a leader who leads an organization like the ASI with action". He continued, "look at these tents", and pointed at the tents of the Dholavira camp site where we were sitting and talking, "you know, these tents are in tatters, they have been repaired nearly a dozen times and are twenty years old. When we gave a request to the ADG office for new tents, they rejected our application". He went on to give me more examples, which he thought were a direct consequence of a non-technical DG. Of these, an important concern was that of arbitrary transfer of officers from one department of the ASI to another. He explained, "earlier, the DGs were archaeologists, and they knew who each and every officer was. They knew which officers were academic and which ones were fieldworkers, and thus were able to put the correct person in the right departments. But now it is chaotic. The DG and all the other IAS officers don't have day to day contact with their officers and so they don't have a clue about individual abilities and things are all muddled."

Despite the discontent among the junior ASI personnel, nothing much had changed, since the "IAS mafia," according to one informant, had a very strong lobby in the government and "they have a reputation of not parting with a post once they have captured it". The first recommendation that the Lal Committee submitted to the Government of India was the restitution of the DG post to the departmental archaeologist officer, as the committee believed this to be the crux of the problem that ASI was facing. They noted in their recommendations:

> For heading a highly professional organization, namely that of archaeology, it is necessary that the incumbent is an eminent archaeologist having wide experience in the discipline, with requisite administrative experience, rather than an administrator

with some knowledge of archaeology. Ideally the post of DG ASI should be filled up by promotion of departmental officers. Filling up the vacancy through deputation totally deprives the departmental officers of an opportunity, as the experience of the last ten years shows. Hence, deletion of the method of deputation" (Lal 2001: 8).

The report is very critical of the fact that a non-archaeologist DG had been head of an archaeological organization. It voices a similar concern that the archaeologists I spoke to did. In an incredibly stringent and severe condemnation of the policy the committee acerbically notes:

> First and foremost: For nearly a decade now the successive occupants of the chair of the Director general of this great archaeological organization have been bureaucrats who, to say the least, were far removed from archaeology. These persons, being non-technical, have clearly been utter failures in providing any leadership in matters of excavation, conservation, research, and other technical matters. Some of the members of this Committee, during visits abroad, have in fact been questioned by senior fellow archaeologists in various countries: "Is your country bereft of archaeologists that you have one bureaucrat or another as the Director General?" What a shameful situation to face, merely on account of the continued indifference of the Government to such vital a matter. Even the British rulers, who were known for their despotic ways, never dared to appoint a non-technocrat as the Director General of Archaeology (Lal 2001: 30).

However, nothing was done, so much so that in the fourth instalment of the committee's report, in the covering letter to Ananth Kumar, the then Minister of Tourism and Culture, Dr. B.B. Lal laments:

> "Finally the Committee would like to express its deep anguish at the fact that its recommendation about the appointment of an archaeologist as the Director General, sent to you as far back as May 2000, has borne no fruit so far. If that is going to be the ultimate fate of our recommendations, what was the great idea in appointing a Review Committee and spending money on it and, more importantly wasting the precious time of so many senior archaeologists of the country?" (Lal 2001: 22).

Finally, in 2004, for the first time, Dr. R.S. Bisht was able to break the "iron hold" of the IAS by becoming the first non-IAS Jt. DG in fifteen years. My informants told me this was a historic event that had been possible only because Jagmohan, who was the Minister of Tourism, the ministry under which the ASI fell, was close to Dr. Bisht. They explained that

because Jagmohan was a Minister he was able to negotiate with the IAS and get this post back for ASI archaeologist-officers. With this change, one informant explained: "both the two posts of the Jt. DG will now be back with the ASI." He did not believe that things would change radically but considered it "at least a step in the right direction."

Genealogy of Mortimer Wheeler and the Excavation Branch

One late evening after the excavation for the day had got over, while sitting on plastic chairs drinking tea served by a turban clad, trouser wearing laborer, I had a conversation about Wheeler's contribution to and impact on the ASI, with the Co-Director of the Dholavira excavation and a couple of Asst. Archaeologists. In the course of the conversation that was mostly related to Wheeler's methodological impact, the Co-Director explained to me that: "we form a direct lineage with Wheeler. Look, he trained Dr. B.B. Lal, Dr. Thapar, (ex DG of ASI) and they in turn trained almost all the senior archaeologists who have worked in the ASI along with Dr. Bisht. We in turn have been trained by Dr. Bisht, who is our guru; in effect Wheeler is our guru." This reverence for the teacher was not unusual in the ASI. I had many a times seen students and even senior archaeologists touch Dr. Lal's and even Dr. Bisht's feet as a mark of respect and acknowledgement for their guru.

This connection between Wheeler and the academic basis of the Excavation Branch was an important element of the identity of the archaeologists working in the Excavation Branch as scientists. They considered themselves to be direct descendents of the work ethic, the academic excellence, and the scientific excellence that Wheeler epitomized. Wheeler was an important figurehead for them: "he gave a new life to the ASI, and made it a truly scientific organization." This was not an unusual sentiment, as most archaeologists in India, including those in the university departments considered Wheeler to have provided a new direction to the ASI (see Clarke 1979; Chakrabarti 1988: 173-188; Paddayya 1995: 134). But for the ASI archaeologists, to be part of an Excavation Branch rather than any other department of the ASI, was considered prestigious because it was a marker of their academic acumen. "At least earlier", explained an ASA "all those archaeologists who had an academic bent of mind were sent to the Excavation Branch. But since the IAS has taken over the reins of the ASI, things have gone to dogs [*khak ho gaya*]. Now they send any one [*kisi bhi aere gaire nathu gaire*] to the Excavation Branch. That's why things are in such a bad state."

The Excavation Branch owes its present character to the foundation that was laid by Mortimer Wheeler during his tenure in 1944-1948. The Excavation Branch had been fundamental to the working of the ASI since the late nineteenth century and was central to the archaeological discovery of the Indus Civilization in the 1920s. However it was closed in 1932 due to general retrenchment and was only re-established after Wheeler joined ASI in 1944 (Wheeler 1946a: 2). In his "Staff Memorandum No. 1", issued in May 1944, which was the first official communication to his staff in the ASI, elucidating the reason for the re-establishment of the Excavation Branch, Wheeler notes that:

> "I am constituting forthwith an Excavation Branch of the Archaeological Survey, under the immediate and permanent control of an officer of the grade Assistant Superintendent. This Branch, supplemented as needed or as opportunity arises, by additional specialists and by students, will in future be responsible for most of the fieldwork carried out by the Survey, and will form the cadre of a training-school for Indian field-archaeology" (ASI Archive Collection New Delhi, File 33/24/44; 1944).

Wheeler strategically framed the re-establishment of the Excavation Branch as obtaining out of intellectual necessity for the resurgence of ASI as an academic organization. He provided a formidable archaeological problem which he believed, could only be solved by a disciplined ASI, especially the newly formed Excavation Branch, through scientific excavation. This was the chronological problem. His intention was to solve the temporal dysfunction of the Indian past, both in the Northern plains where the Indo-Gangetic civilization had thrived and in the Southern plateau and the coast where multiple kingdoms had risen and fallen. In both cases, Wheeler's intellectual interest was driven by a desire to uncover the chronological absence that marked the archaeology of these regions. The problem in the north according to Wheeler was much simpler and involved just uncovering the temporal hiatus between the Indus Civilization (third millennium BCE) and the North-western kingdoms of the sixth century BCE. The dates of both the periods were archaeologically available through comparative and analogical dating with the Western world- the Indus civilization with the Mesopotamian and the later settlements with the Achaemenid Empire. For the south, the problem according to Wheeler was far more difficult, because unlike the north, the settlements south of India did not have any comparative temporal analogical relationship with the western world. As Wheeler explained it: "In the south of India the archaeological problem is, in a sense, vaster still. There we have no dated contact with ancient Mesopotamia, no intrusive Persian Empire...[F]or earlier periods material is abundant, its inter-relationship unknown. It is a jumble of words

with no consecutive meaning. But here again, planned work can gradually bring order and significance into chaos" (Wheeler 1955: 188). The importance of correlating Indian sites with Greco-Roman artifacts became one of the most important agendas for Wheeler. He notes in one of his texts that in order to deal "with the 'Dark Ages' of the Vedic period, the first requirement was to determine its delimiting phases with all possible exactitude" (Wheeler 1949: 5). This was the research focus of all the excavations that Wheeler undertook in India (Taxila, Arikemedu, Bhramagiri and Harappa) during his four-year tenure as the Director General of ASI. These excavations were conducted by the newly constituted Excavation Branch. In his "Staff Memorandum No. 1," Wheeler explicitly puts his point across in no uncertain terms to his staff members:

> "Here then are two great problems which demand attention...The work may not be immediately spectacular in the popular sense of the term, but, properly planned and controlled, it is capable of adding notably to our knowledge of components of Indian civilization. And that is a basic function of the Archaeological Survey...We must move forward; the Department must be fully prepared for its role in the India of the future...Our responsibility, as guardian of an important part of this heritage, is proportionately urgent" (ASI Archive Collection New Delhi, File 33/24/44; 1944).

This research project directed nearly two decades of archaeological work in India. In a way, the Saraswati Heritage Project was a continuation of this research goal that Wheeler delineated more than half a century ago.

For archaeologists in the ASI, it was the Excavation Branch that distinguished the ASI from other departments of the Central government. "If it was not for the archaeological knowledge that we produce, we could have been any other central government bureaucracy, like the irrigation department. We are different because we do science" explained the AA at Dholavira. This ability to "do science" was important for the archaeologists as well for the ASI, as it legitimized the organization as a scientific one. Although bureaucrats were primarily a part of the postcolonial governance project, these archaeologists wanted to differentiate themselves from other bureaucratic organizations. Theirs was an attempt to create a special identity as scientists, which was "much more than governing a district or making roads and dams". It was because of this that Wheeler's legacy was important to them, for it was Wheeler who had given the ASI the belief that they were scientists, that they were involved with the national task of giving the nation a past. Wheeler infused the aspiration of a country soon to be

independent with the rigor of scientific discipline. The "Wheeler connection" was very important for the ASI archaeologists and the trope of science introduced by Wheeler into the disciplinarian practice of the ASI, was adhered to loyally. Almost all the archaeological officers who worked in the ASI had been trained in the Institute of Archaeology. The Institute of Archaeology was modeled after the Institute of Archaeology in London and had been inaugurated by Wheeler in 1959. Its establishment was the direct product of this combination of a national and a scientific task, and cohered perfectly with the new nation's adoption of a scientific agenda. It was in the same decade that the famed Indian Institutes of Technology (IITs) were established in different parts of the country. The archaeologists were viewed as the technical staff of the ASI and the Excavation Branch was also considered to be the "technical branch" of the ASI, where the skill of an archaeologist was framed within a folk theory of technology and science.

The Excavation Branch

In the ASI, the task of conducting archaeological excavations was the exclusive prerogative of the Excavation Branches, located in different parts of the country - Nagpur (Ex. Br. I), Delhi (Ex. Br. II), Patna (Ex. Br. III), Bhuvaneshwar (Ex. Br. IV), Baorda (Ex. Br. V). The job of these branches was to conduct excavation of the archaeological sites in their designated geographical territories and write reports of the excavations they conducted. However, the various Circle offices and the office of the Director General (DG) of the ASI could also conduct excavation along with the State Archaeological Boards, which did not come under the purview of the ASI[41]. Till 1947, the Excavation Branches did not exist. The Circles and the DG office carried out the excavations, but the vigor to explore, discover, and excavate archaeological sites in the newly independent country led to an unprecedented amount of archaeological work in the early years of post-independence ASI. The Nagpur Excavation Branch was the first to be opened and then subsequent branches were started. During my fieldwork, I came to know that a new Excavation Branch. V1 was being planned in Mysore in South India. These excavation branches were located in different parts of the country and were assigned the job of conducting exploration and excavation work based on the initial

[41] However the Lal Committee was unhappy about the Circle office conducting archaeological excavation. In its report it argued that only the Excavation Branches should be given permission to conduct excavations. They noted: 'In so far as the Circles are concerned, they should limit their excavation activity only to scientific clearance around monuments in order to expose the buried parts of the complex" (Lal 2001: 187)

exploration. The task of the excavation branches was exclusively epistemic:

- Carrying out problem-oriented survey including exploration and excavation of ancient sites and mounds;
- Research on the ensuing exploration and excavation work;
- Preparation of reports based on the fieldwork;
- Interaction with various universities and research institutions; (ASI website)

Within the ASI, the Excavation Branch was the institutional framework with the sole responsibility of producing archaeological knowledge. Its administrative hierarchy was structured so as to optimize the efficiency of the knowledge production mechanism within the postcolonial bureaucratic setting. It was conceived as a scientific organization that would fulfill the function of an academic body in a largely bureaucratic institution. It was envisaged as a unit of the ASI which would be kept away from conservation, heritage management, and other administrative activities of the ASI and would be exclusively involved with the discovery of new archaeological sites, their classification, categorization within the body of academic literature on Indian archaeology and history, and most importantly with the excavation of archaeological sites. Senior ASI archaeologists told me that the Excavation Branch was considered as that section in the ASI where the most "brilliant" archaeologists with an "academic bent," would be posted. Here brilliance and academic inclination were defined not by the amount of academic work that a particular archaeologist had published but by the performance of an archaeologist in the field. I was told by a number of informants that an archaeologist's brilliance was judged more by his ability to manage staff and labor in the field than his/her proclivity towards things archaeological. Wheeler had aptly termed this ability of administrating staff and labor in an excavation site as "man-management" (Wheeler 1954: 173). Although the Excavation Branch was framed as an academic oasis in a bureaucratic environment, its organizational structure was as hierarchical and bureaucratic as any other department of the ASI.

Before I came to the field, I had done a close reading of Wheeler's *Archaeology from the Earth*, because I knew the profound impact this text had had on the excavation practices of the ASI. So I was not surprised to see that not only did the archaeological practice of the ASI faithfully follow the Wheeler Method but also the organizational structure of the Excavation Branch was an exact replica of the model found in Wheeler's text. In the ASI, it was the

Superintendent Archaeologist (S.A.) who was at the helm of the Excavation Branch hierarchy. He was usually supported by a couple of Deputy Superintendent Archaeologists (Dy.SA), and around three to four Assistant Superintendent Archaeologists (ASA) and Assistant Archaeologists (AA). These were the "officer" rank staff members of the Excavation Branch, and comprised the upper layer of the hierarchy of the Excavation Branch bureaucracy. One of the Dy.SA had the additional responsibility of the Account Officer. The next layer consisted of the technical staff, which consisted of the Senior Photographer, Senior Artist, Senior Draughtsman, Senior Surveyor, followed by their juniors. Each branch would have around 6-8 such technical staff members; their numbers would vary because of recruitment practices in the AS1. The next layer consisted of the non-technical administrative staff of the Excavation branch consisting of a Storekeeper -- who was basically involved in the procurement and maintenance of excavation tools and equipment; Lower Division Clerk (LDC), Upper Division Clerk (UDC), who were involved with the maintenance of accounts and the general administrative work in the Excavation Branch office, along with a stenographer. Since the Excavation Branch was recognized by the ASI as primarily an organization involved with the production and dissemination of archaeological knowledge, each branch had its own library and a librarian. The last layer in the bureaucratic hierarchy consisted of what was commonly called the Class IV staff, which consisted of a couple of Office Assistants, a driver, a sweeper, and a host of daily-wage employees. The daily wagers [*dahari*] were important members of the Excavation Branch bureaucracy as they were the lowest paid and did the most menial work. They primarily consisted of relatives of the staff members who would work on daily wages, sons, distant relations, close family members. These were temporary workers, called "casual supervisors" and were hired on a daily basis. I was told, some had worked for decades, on this provisional status, others would be later hired when a retirement occurred. During the excavation season, graduated students from the Institute of Archaeology would also be employed as excavation supervisors in the field, and were also paid daily wages - albeit at the pay scale for technical staff. This organizational structure of the Excavation Branch faithfully mimicked the one Wheeler had elucidated in his *Archaeology for the Earth*: "The staff of an archaeological excavation on any considerable scale includes a director, a deputy director, a supervisor for each area under excavation, a trained foreman, a small-find recorder, a pottery assistant, a photographer, a surveyor, a chemist, a draftsman...only an ignorant critic could protest that the list is excessive" (Wheeler 1954: 153). Wheeler continues and explains the necessity of a well-staffed excavation team: "large and relatively costly expeditions in the past

have failed of their duty through false economy or lack of prevision in this all-important matter of staff " (Wheeler 1954: 153).

Within the institutional hierarchy, the Excavation Branch was independent. The SA of the branch had the right to conduct an archaeological excavation anywhere in India. I was informed that despite this freedom, it was frowned upon if an Excavation Branch undertook any archaeological work beyond its designated territory. The reason for this was both logistical and territorial, it was explained to me. It was a wide-ranging belief that an Excavation Branch should conduct archaeological work in the areas close to the Branch headquarters since it was logistically easier to move equipment and supplies, and so that the staff could visit the Branch headquarters and their homes from the field as often as necessary. The Excavation Branches were spread throughout India in such a way that each branch had ample area to conduct archaeological excavations. Over the course of the past 2-3 decades each Excavation Branch had developed an area of expertise – some specialized in historical archaeology of medieval India, others on Buddhist archaeology, and some on Harappan Archaeology and prehistoric archaeology. Although such expertise was not formally stated, it was informally associated with a given Branch. These specializations were related to the territory that an Excavation Branch was responsible for. For example, the Patna and the Bhuvaneswar Excavation Branches were experts at excavating Buddhist sites as they were located in eastern India where a host of Buddhist sites were located. The Delhi Excavation Branch was primarily involved in excavating historical sites related to medieval Muslim and pre-Muslim habitation in the Gangetic valley in North India, while the Baroda Excavation Branch specialized in the excavation of the Harappan sites since it was situated in Western India where most Harappan sites were located. This idea of specialization was built upon the pervasive perception that Indian archaeological heritage was very complicated and diverse and each geographical and topical area required years of expertise to be understood. Specialization was also centered around the idea of reputation. For example, the Nagpur Branch was considered to be the best Excavation Branch, and it had a reputation for excavating Harappan sites, even though the Harrapan sites it dug were far away from its territorial bounds. It was associated with some of the best Harappan excavations conducted in post independence India at Surkotada in Gujarat and Kalibangan in Rajasthan. Thus the decision to excavate at a particular site was not solely directed by academic curiosity and "problem oriented" research questions, but also dictated by logistical issues, ideas of academic territoriality and the self-

perception of specialization and reputation of each Excavation Branch.

However the SHP was an exception to the above rule. For the first time in the history of the ASI, I was told - all the Excavation Branches were ordered to abandon the projects they were involved in and to dig the sites that they had been allocated by the office of the Director General. The SHP was the political project of the ASI and the chief architect of the project, Dr. R.S. Bisht, who was the excavation director of Dholavira, was then the Joint Director General (Jt. DG) of the ASI and also the Director of the Excavation and Exploration Department of the ASI, as well as the Director of the SHP. Bisht was close to the Cabinet Mister of Tourism - Jagmohan, and exercised tremendous clout in the ASI during his tenure as a Jt. DG. By October of 2003, orders had been sent to all the Excavation Branches to discontinue all the previous work they had been involved in to participate in the SHP.[42] As I have explained in Chapter 1 the SHP was envisaged as a large national project with more than fourteen excavations planned. However, at the time the project commenced in December of 2004, work had begun only on the sites that the Excavation Branches had been allocated. Eventually only these sites were excavated as part of SHP in the ensuing two seasons. Within the bureaucratic structure of the ASI, it was virtually impossible for any of the Excavation Branches to refuse the order to participate in the SHP project and pursue another excavation elsewhere. Various informants I spoke to were unhappy with this situation. For some, it was the issue of expertise and specialization while for others, the order violated the freedom of the SA of the Excavation Branch to determine its own research agenda and excavation projects. For example a number of ASI staff members of the Patna and Bhuvaneshwar Excavation Branches were unhappy that they were being asked to excavate sites that lay virtually at the other end of the country. They explained that the situation was bizarre as they were ordered to go more than 1500 km away from their Branch headquarters, and that in the entire history of their branch, there was no one who had any experience working on Harappan sites. Within the logic of specialization, the expertise of these two branches lay in the excavation of Buddhist monuments and sites in eastern India. Their professional reputation, as one informant explained, was built on the excavation of Buddhist sites; "we are experts on the archaeology of

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[42] This idea was first discussed in the Standing Committee of CABA meeting on 19.08.2003, where Dr. Bisht argued "The Excavation Branch VI which has been created recently is yet to be activated. The other five Excavation Branches can be concentrated on the River Sarasvati and Branch VI can be accommodated for the investigations of Arikamedu".

Buddhism, we don't have any clue about black & red ware or perforated pottery."

When I spoke to the SA of the Baroda Excavation Branch about this issue, she told me that "I will go wherever my DG orders me to". This seemed more like an official response to my inquiry. When I spoke to members of her staff, they told me that she had been forced to dig the site of *Juni Kuran* by her seniors. She had wanted to conduct the third season of excavation at the site of *Hatab*, an early historic site on the coast of Saurashtra, but the Jt. D.G. had compelled her to dig at Juni Kuran, as part of the SHP. I was not sure about the seriousness of the tension between the various Excavation Branches and the Jt. D.G. on this issue, during my first season of fieldwork. But in my second season (2004-05), the situation changed, as the BJP led government lost the general election in October 2004, which led to the Minister of Tourism, Jagmohan, relinquishing his position and Jt. D.G. Dr. R.S. Bisht retiring. In this season (2004-05) the Excavation Branches of Baroda and Bhuvaneshawar, both independently decided to discontinue excavating at Juni Kuran and Tarkanwala Dera / Chak 86, respectively, even though the original plan had been to excavate each of these sites for between 3 and 5 years. I was informed that both the sites were rich enough for a couple of more seasons of work. By my understanding, Juni Kuran was a very rich site and in the excavation style of ASI, required more than 5 years of work. But both Branches decided not to continue excavation at these sites. When I made enquires, I was told that the SA of both these branches were not interested in the first place in excavating SHP sites; for the Bhuvaneshwar Excavation Branch it was a matter of logistics, and for the Baroda Excavation Branch it was a matter of autonomy. In the case of the Bhuvaneshwar Excavation Branch, I was informed that the Branch staff had been very miserable as they were forced to work so far away from home. The SA who was the Excavation Director of *Tarkanwala Dera / Chak 86* excavations had stayed on the sites only for a few days a month, and had supervised the excavation mainly through telephone conversations. In Baror, during the first season, the excavation had been conducted in the absence of the SA, who was also the SA of the Bihar Circle, and she had only come to the site when it had been inaugurated. The excavation was chiefly being conducted by the two AAs, and a couple of senior very experienced ex-students who were constantly at the site. While talking to an Assistant Superintendent Archaeologist, who was bitter about this forced excavation, more than a thousand kilometers away from his home, he told me that "there was no need for Bisht to order the Bhuvaneshwar, Patna and Nagpur Excavation Branches to leave their home areas to excavate in SHP sites. He could

have requested the Archaeological Dept. of Haryana, Rajsathan, and Gujarat to excavate these sites or he could have just constituted a new SHP excavation department, and deputed archaeologists and staff from various Circle and Branch offices."

Superintendent Archaeologist

The Superintendent Archaeologist (SA) was the "boss" of the Excavation Branch, as most officers below would call him or her. S/he usually had a PhD in archaeology, had spent a number of years working on ASI archaeological sites, and was usually exceptionally well versed with the ASI's way of doing archaeology, which, as I shall show in my next chapter, is a culturally specific scientific practice. S/he was considered to be the *kartha-dharta* [sustainer] of the branch, being both the bureaucratic and the academic head of the Branch. During an archaeological excavation, the SA of the Excavation Branch would also be the Director of the Excavation, and all publication related to the excavation or exploration conducted by any member of the Excavation Branch had the SA as the first author. The Excavation directors that I met seemed to fulfill the qualities of the model director of the excavation that Wheeler describes: "It would be easy to be trite in describing the qualities of the director. It goes without saying that he must have the combined virtues of the scholar and the man of action...the director sets standard of achievement and must know enough to impose his standards without questions on his experts" (Wheeler 1954: 154). This was not different from what Flinders Petrie said, writing more than half a century ago in the first chapter of his seminal introduction to archaeology devoted to the character of the excavator: "Scheming how to extract all that is possible from a given site, how to make use of all conditions, how to avoid difficulties; and training laborers, keeping them all firmly in hand, making them all friends without allowing familiarity, getting their full confidence and their goodwill; - these requirements certainly rank high in an excavator's outfit" (Petrie 1904: 4).

I was informed that when the earliest Excavation Branches were formed, the post of an SA was considered very prestigious as it reflected the SA's academic credentials, but lately the SA of an Excavation Branch was considered to be a "punishment posting." A punishment posting was a term very often heard in the corridors of offices of postcolonial bureaucracy in India. It referred to the posting of government employees to posts which were not desirable either because they were located in geographically remote areas, or because they were not prestigious or lucrative positions. For example, within the ASI, to be posted as the Director of

a remote Site Museum away from urban centers would be considered a punishment posting. Site Museums like Lothal (Gujarat), Kalibangan (Rajasthan), Aihole (Karnataka), were often cited as examples of punishment postings, since they were very far from urban centers. "These are great postings for a bachelor AA or an ASA," explained a DySA, "but for a married man with children, these are most unsuitable. There are no good schools, houses, or even proper bathrooms".

Posting as an SA of the Monument Section in the DG office, or an SA in the ASI Library, both of which were located in New Delhi were also considered as punishment postings. Despite being well located, these positions did not have enough responsibility and were not considered "powerful." The most lucrative postings were those that were powerful and heavily funded, and contained the potential for "making money". And it is for this reason that the posting as an SA of an Excavation Branch was considered to be a punishment posting. It was not lucrative. This was emphasized by a number of informants, whenever the issue of corruption cropped up in conversations. On further enquiry, I was told that unlike an Excavation Branch, which had an annual financial outlay of less than 10 million rupees, the Circle offices had a financial outlay of more than 50 million rupees, and this was the major reason that the posting as an SA of a circle was a more coveted position. "Corruption happens in both places, but the potential for corruption is virtually unlimited in a Circle", explained an Asst. Archaeologist to me, while we were taking a daily tour of the excavation site. "That's when you make the most money in your career in the ASI. As a matter of fact, the SA of a Circle does not want to be promoted, because as a Director in the DG headquarters, the possibility of making money declines drastically." During the two years that I did my fieldwork, more than six Directors who were at the helm of the ASI, had numerous corruptions charges leveled against them and the CBI (Central Bureau of Investigation) was conducting investigation against them.

"The SA of a Circle" an informant told me, "in the old days was more powerful than the Chief Secretary of the State, as the Circle consisted of not just one federal state but at least two to three states. He had *power and prestige*. But things have changed lately". The power and prestige, I was told was not limited to having the privileges that equaled those of the topmost bureaucrat of the federal state, which included obvious markers of governmental perks, but it lay in the power of jurisdiction over a huge geographical area and the funds allocated to each

Circle. The informant who was an Asst. Archaeologist explained that "the SA of a Circle was earlier so prestigious that when he went on a tour, the District Collector and the District Magistrate would accompany him, but those days are now gone and the things have changed." When I asked him what he meant by things having changed, he said: "now each state has a Circle, because protected monuments have increased, and also because our pay scale has decreased"

I was informed that there was a crisis of staff members and officers in the ASI and that the Excavation Branch was the most neglected division of the ASI and most of the Branches were understaffed. For example, the Delhi Excavation Branch had only two archaeologists other than the SA, both of whom were of the AA rank. During the 2003-05 season, the Patna Excavation Branch was digging in the field without an SA; the Excavation Director was a DySA. In these circumstances, the Excavation Brach would employ ex-students, who had graduated from the Institute of Archaeology to supervise most of the excavations in the field. These were students who had studied at the Institute of Archaeology. Along with these ex-students, the continuing students of the Institute would form the lowest layer of the ASI's institutional hierarchy in the excavation site. The student archaeologists were a constant fixture in ASI excavation sites since 1944 when Wheeler, for the first time in the history of Indian archaeology, conducted a field school at Taxila. For Wheeler, the student archaeologists were more than an extra handon an excavation site; they fulfilled an intellectual purpose at the site: "They impose a constant need for clear exposition and therefore clear thinking. They ask simple, awkward questions, which have to be answered convincingly or with frank and wholesome admission of ignorance. You can't fool them. They are the friendliest and most stimulating of critics, and the best of them rapidly become the most co-operative of colleagues" (Wheeler 1954: 153).

It was the colonial bureaucracy that recognized the importance of archaeological sites in India and formulated complex bureaucratic procedures to allow access for facilitating archaeological excavation. The postcolonial ASI had further strengthened the bureaucratic process through which archaeologists could excavate archaeological sites in India. Archaeologists in University departments, who are at least lecturers, with a PhD in Archaeology, archaeologists of the State Archaeological Board, at least of the rank of S.A or above, and archaeologists of the various Circles and Branches of the ASI at least at the rank of

Deputy S.A. could apply for a license to excavate an archaeological site in India. These sites could be Centrally protected or State protected monuments or newly discovered sites. The proposal had to be sent to the Excavation and Exploration Dept. of the ASI, which then forwarded the application to the Standing Committee of the Central Advisory Board of Archaeology, who would advise the DG of the ASI whether to issue a license to the would-be excavator. The Central Advisory Board of Archaeology was formed in 1944 and consisted of senior archaeologists from various University departments senior bureaucrats of the ASI, and retired bureaucrats of the ASI, most of whom had experience of conducting archaeological excavations.

From the above description of the ASI structure, it might appear that each Excavation Branch would be associated with one archaeology site, from the beginning to the publication of the report. But this was paradoxically not the case. The excavation site was not associated with an Excavation Branch but rather with the Excavation Director, who was usually an officer at the level of SA or higher. For example, the Baroda Excavation Branch undertook the Dholavira excavation, which commenced in 1991, because the Excavation Director of Dholavira was Dr. R.S. Bisht who was then the SA of the Baroda Excavation Branch. But in 1994, he was transferred as the Director of the Institute of Archaeology, and then the excavation department of the Institute conducted the Dholavira excavation. Then he became the Director of the Excavation and the Exploration Department of the ASI and during that period, the Dholavira excavation team consisted of members from the Director General's office in New Delhi where the Excavation and the Exploration Department was located. Finally in 2002, Dr. R.S. Bisht became the JtDG of the ASI and then the members of the Dholavira excavation were drawn from various Branches and Circles. When I reached Dholavira in the season of 2003-2004, the Co-Director of the Dholavira Excavation was a DySA from the DG office; there were four Asst. Archaeologists -- one from the Baroda Circle, two from Dehradun Circle, and one from the Excavation and the Exploration Department in the DG office. The technicians were also drawn from various departments, Branches, and Circles. This was not just the case of the Dholavira but excavations at other sites like Rakhigarhi and Surkotada were also intrinsically linked to the individual excavator. One of my informants, an ASA informed me that this had led to a "cult of proprietership", where the individual Excavation Director completely controlled access to the excavation site and "had nearly dictatorial control over the material excavated" (*Excavation Director ki dictatorship chalti hai, pura control rahta hai*). He

explained that this was one of the prime reasons that excavation site reports were not published. Thus the Excavation Branches were bureaucratic institutions of professional archaeologists and technicians who worked mechanically and were supposed to employ their skills wherever they were ordered to. This form of institutional ethic was a remnant of the colonial military structure that ASI emerged from. In the ASI, excavation was exclusively the articulation of the intellectual inclination of the Excavation Director; other members of the Excavation Branch were merely technical help. Although this "cult of proprietership," was not articulated as an official policy, the bureaucratic system of the ASI perpetuated this connection between the individual excavator and the archaeological site.

The cause for this can be traced to the relation between knowledge production and the individual within the organizational setting of the ASI. The ASI was not an academic university - it was primarily a bureaucratic system. On the one hand, it aspired to produce archaeological knowledge like an academic body, but on the other, it also wanted to execute a project like a governmental organization. In its attempt to simultaneously fulfill both goals, it was unable to do justice to either of them. Genealogically, the proprietary relation between the excavation site and the individual excavator was formed during the reign of the first DG of the ASI – Sir Alexander Cunningham, a military officer with an academic inclination – similar to many other earlier colonial academicians in colonial India. He instituted a system in which the Excavation Director was at the helm of affairs and had complete control over the excavation and where other members of the excavation team were his staff rather than his colleagues. The excavation/exploration work was carried out in a rigid hierarchy, with the Excavation Director treating the rest of the excavation team as his subordinates, where the subaltern laborers were at the bottom of the hierarchy. Mortimer Wheeler further strengthened this system, conflating the military discipline with professional ethic, and virtually sealing the unequal relationship between the Excavation Director and the rest of the excavation community. Wheeler's disciplinarian model of archaeological excavation was very much in place at the time I arrived in the field.

Assistant Archaeologists

At the bottom of the ASI hierarchy was the AA. These were the youngest officers in the ASI, usually students who had recently graduated from the Institute of Archaeology in Delhi and they formed the base of the ASI officer cadre hierarchy. These were truly the officer base of

the ASI. During my fieldwork I spent a lot of time with these officers. They were the ones who ran the archaeological excavation at the site; the senior officers, were more of administrative and research overseers. The AAs spent all their day on the excavation site controlling and supervising the excavation. They functioned as the conduit between the laborers and the senior officers. Along with the technical staff members of the excavation team, the AAs considered themselves to be the ones who "sweated in the field" [*hum field main pasina bahate hai*]. All the AAs I spoke to complained that they worked the most in the field, wrote reports and articles, but did not get any credit. Citing as an illustration the two volume 2003 Ayodhya Report (which was tabled to the Lucknow Hight Court in less than two months after the excavation), an AA who worked at the site and also wrote the report confided to me "At Ayodhya, the majority of the chapters of the report, other than the Introduction and Conclusion, were written by us (AAs). The deputed SAs only read through it and did the grammatical and stylistic editing. We did the hard work. We did the tabulation, the pottery analysis, and the analysis of the architectural members and antiquity analysis, but were only credited as one of the many authors of the report."

The professional dissatisfaction among AAs also emerged from one of the thorniest issues in the career of an AA - salary. I was told that "until a couple of decades ago, the salary of the AA was more than that of a university lecturer, then it became same as a university lecturer, and now it has become less than that a university lecturer." This disparity in salaries has also been noted in the Lal Committee report, which states:

> "The entry grade to technical posts in the Archaeological Survey of India is Assistant Archaeologists (1640-2900)...A demand for a higher pay scale at this level has been made. The Archaeological Survey of India, in their official memorandum, have stated that keeping in view the educational qualifications, nature of duties and responsibilities attached to the post of Assistant Archaeologist and the all India Transfer liability, the present pay scale of Rs. 1640-2900 for the post is not commensurate with job requirements. Further, the initial entry in other sister organizations like Geological Survey of India is in the pay scale of Rs. 2000-3500. Consequently, the ASI is experiencing difficulty in appointing qualified persons. They have, therefore, recommended upward revision in the pay scale of Assistant Archaeologist" (Lal 2001: 43).

Earlier this post was also called the Technical Assistants (TA), but this changed in the late

1970s when the recruitment program for juniormost officers changed. Other than AA, all other senior ranks of the ASI were recruited through the central government recruitment agency - UPSC (Union Public Service Commission).[43] Till the late 1970s, the post of the AA, then called the TA, was filled through internal recruitment practices. But in the 1970s, the recruitment at the AA level was taken over by another central government recruitment agency called the Staff Selection Commission (SSB).[44]

An ASA told me that before the SSB took over the recruitment regime of the ASI, the AAs were recruited through informal channels, which he said was commonly known as the "*Bhatinda Line*". This informant was one of the first cohorts of AAs to be recruited by the SSB and was proud of this achievement. He explained that the "*Bhatinda Line* was the backdoor channel of getting a job in the ASI before the SSB recruitment kicked in". The *backdoor channel* worked in a casual way and "most senior officers who have risen to high ranks from the post of the TA entered through the *Bhatinda Line*. Even today, all the technical

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[43] The Union Public Service Commission had been established under Article 315 of the Constitution of India, and its legacy goes back to the Federal Public Service Commission under the Government of India Act 1935. However, the first Public Service Commission was set up on October 1st, 1926, by the colonial government as a product of the slow transformation of an erstwhile majority British colonial bureaucrats to a more native one. With the institution of the new Constitution in 1950, the Federal Public Service Commission was accorded a constitutional status as an autonomous entity and given the title – Union Public Service Commission. It is instrumental in selecting persons to man the various Central Civil Services and Posts and the Services common to the Union and States called the "all-India Services". The UPSC has been entrusted with the following duties and role under the Constitution:

> "1. Recruitment to services & posts under the Union through conduct of competitive examinations; 2. Recruitment to services & posts under the Central Government by Selection through Interviews; 3. Advising on the suitability of officers for appointment on promotion as well as transfer-on-deputation; 4. Advising the Government on all matters relating to methods of Recruitment to various services and posts; 5. Disciplinary cases relating to different civil services; and 6. Miscellaneous matters relating to grant of extra ordinary pensions, reimbursement of legal expenses etc" (http://www.upsc.gov.in/; accessed on 22 May 2006).

[44] Earlier known as the Subordinate Services Commission, established in1975. This Commission was subsequently renamed as Staff Selection Commission in 1977. It was formed after the Estimates Committee of Parliament, in its 47th Report (1967-68) recommend its formation because, as its website notes: " The Administration Reforms Commission (ARC), in its Report on Personnel Administration, drew pointed attention to the fact that bulk of the staff of the government at the Centre and in the States belonged to class III and class IV categories. Referring, in particular, to the identical nature of qualifications stipulated for entry into such posts in various offices, the Commission advocated pooling of the requirements of non-technical posts by different departments and selection of personnel either by joint recruitment or through a recruitment board". The Commission is an attached office of the Government of India, Ministry of Personnel, Public Grievances and Pensions. The Commission "now makes recruitment to Group B (Non-Gazetted) and non-technical group 'C' non-gazetted posts in the Ministries/ Departments, attached and subordinate offices of the Government of India" (http://ssc.nic.in/about.htm; accessed 22 May 2006).

staff - photographer, draughtsman, surveyor - enter the ASI through the *Bhatinda Line*". He further illustrated "for example, you know Rastogi*ji*," the ASA referred to a DySA who worked in the DG Headquarters, "his father was in the ASI as a draughtsman and soon after his BA, through his father's connection, he got a daily wage job as a site supervisor in an excavation site. He worked at the site for a couple of years and was then given a permanent job as a TA". Speaking to other informants, I was came to know that this had indeed been standard practice for the recruitment of the AA before the SSB took over and earlier some of these TAs had even risen to the ranks of Directors and ADG.

Before I proceed further it is important to explain the term *Bhatinda Line* and the cultural politics of its application in the conversation above. It was a condescending colloquial term used for a train line between Delhi and Bhatinda- an industrial town in Punjab, northwest of Delhi. On this line run daily commuter trains (inter-city Express) ferrying Blue-collar workers from the hinter land to Delhi. During peak office hours, this train was often very crowed, and it was difficult to get even "standing place". In the ASI context, the term *Bhatinda Line* did not necessarily refer to this particular intercity train but stood for many such overcrowded daily commuter trains, which ran during office hours and fed into Delhi, primarily carrying rural working class population from neighboring states of Rajasthan, Punjab, UP, and Haryana. From the middle class subjectivity inhabited by my informant, this migrant workforce was viewed as rustic, uncouth, uncivilized and incongruous with the cultured harmony that Delhi epitomized. By using the term *Bhatinda Line* for those officers who had been recruited informally into the ASI, this ASA wanted to differentiate himself from them, and to underscore that he had been recruited into the ASI solely on merit and not by exploiting informal networks or bribing anyone. This was also an important distinction made by many AAs to distinguish themselves from other technical staff of the Excavation Branch, like the draughtsman, the photographer, or the surveyor. These staff members were not recruited through the SSB and were considered to be at a lower, non-officer grade in the ASI hierarchy

I knew of number of DySA, ASA and some SAs who had started their career as site supervisors and were later recruited into the ASI as TAs. The *Bhatinda Line* had both its advantage and disadvantage. The advantage was that senior and experienced officers could handpick site supervisor who they thought had a "knack for archaeology." But it also opened avenues for recruitment malpractices. Referring to the intervention of the SSB, a senior SA

noted that this was a "good thing", because "corruption was rampant and all sorts of people without adequate training came into the ASI. You know how coveted a government job is and anyone was willing to pay to get a permanent job in a central government service". Like almost everything in the ASI, recruitment practices amongst technical and non-technical staff members of the Excavation Branch and the Circle offices also produced narratives of corruption.

I came to know through my conversation with the technical staff and the lower staff of the Excavation Branch that, at their grade, "backdoor entry" into the ASI was widespread and that "each post had a price [*har post bikau hai*], from that of a peon to a photographer." A photographer at Baror told me how for the past three years his son had been working as a daily wager in the Excavation Branch "helping with all the tasks of the excavation - photography, site supervision and everything." He remarked that "my son has even done a Diploma in Photography from a reputed institute, but he had not been taken into the ASI." When I asked him why that was so, even though he himself had been in the ASI since more than twenty years, my informant replied, "there are three problems. First, I was not in the good books of the SA, secondly I don't have enough money to pay the cost of the post, and thirdly the SA has his own favorite candidate who obviously paid him the bribe [*sala, khoob paisa khaya SA ne*]. He took two lakhs (Rs. 200,000) to give the post to this favorite candidate."

Although it is impossible to get evidence for such forms of corrupt practices, it is important to report this conversation here because such narratives of corruption were consistently heard at each site and overwhelming in their volume. The narrative of senior officers accepting bribes was one that I heard often whenever the conversation would turn to corruption. I was told that several SAs took bribes during the recruitment of lower grade staff and it was only this general practice that made the SA post lucrative. However, I was always reminded that not all SAs are corrupt: "There is a small number of SAs who are incorruptible [*imandari ki kamai khate hai*]." I was also given narratives about some SAs who would even take their own food during field visits and did not allow their family members to use official vehicles for personal use, a common practice throughout the bureaucratic community in India. But many SAs were notorious for their corrupt practices and "they even have CBI cases slapped against them. As a matter of fact, more than half of the Directors who sit in the DG office have CBI enquires being conducted, on various charges of corruption," exclaimed a AA as he was giving me a

detailed description about how corruption has become a "daily habit" of an ASI officer, "given a chance, I will also not let go of any opportunity of making money," he remarked unapologetically.

Ex-Students

However even after the SSB had taken over the recruitment of AA, I came to know through my conversations with ex-students of the Institute of Archeology who worked on excavation sites as site supervisors, that it was not easy to get a job as an AA in the ASI. These were not the student laborers or site supervisors who did the majority of the supervision work on an ASI site, but these were ex-students who had graduated with a Diploma in Archaeology from the Institute of Archaeology, and worked as "technical daily wagers" on the sites. I was told that this tradition of hiring ex-students as site supervisor was a tradition as old as the Institute of Archaeology. It fulfilled two functions: "first it gave an opportunity to work in the field until we are able to get permanent jobs in the ASI, University Departments, Museums, or the State Archaeology Departments. Secondly, we play an important role in substituting for AAs in large scale excavations," explained an ex-student at Baror with whom I shared my tent. A typical ASI excavation site was a large-scale horizontal excavation where at any given point of time there were around 20 – 40 opened trenches. And there were only 2-3 AAs in each Excavation Branch, so it was very difficult to supervise the whole excavation site with more than 200-300 workers. At sites like Dholavira, Hansi, Baror where students of the Institute were being trained, more trenches would be opened and these would controlled by the students. In Bhirrana when students were not being trained, technical staff likes the photographer, artist, draftsman and the surveyor also coupled site supervisor. Since this shortage of trained archaeologists at the site was a consistent problem, additional help would come from ex-students, but they would only number around 2-3, depending on the number authorized by the DG Headquarters. This was also another example of how the DG Headquarters asserted its control over excavation. A DySA at Dholavira told me that just to get an additional ex-student to work at the site it "took nearly a month and a ton of paper work and justification. And even after that it is possible that our request is rejected."

In the organizational hierarchy of the excavation team, these ex-students had an ambivalent place. On the one hand, they were trained as archaeologists with a Diploma in Archaeology along with an MA and most of those I interacted with were also enrolled in PhD programs.

Therefore they were more qualified than the technical staff of the Excavation Branches. On the other hand, they were just daily wagers, just like the laborers in the field with the difference of technical qualification. It was through conversations with these students that I came to know that despite the taking over of the recruitment job of the AA by the SSB, corruption, favoritism, and nepotism still existed. "Look," my tent mate, an ex-student remarked, "it is not even possible to enter into the Institute of Archaeology without connections. Majority of the students who get into the Institute have some network with someone from ASI. They are taken on merit but connection is crucial."

The Institute of Archaeology was the official training school for ASI archaeologists, especially those who entered the ASI as AA.[45] It was not a separate academic institution, although it was established with that purpose. Spearheaded by Wheeler's vision, the objective of this establishment was to create a cadre of trained archaeologists who would be able to get jobs not just in the ASI but also in other archaeological organizations in the country. Students were not awarded a degree, but given a Diploma in Archaeology when they graduated. However over the course of a few decades, this school has become the "in-house" training centre for ASI archaeologists. The Institute was not an independent educational Institute but was part of the ASI, headed by an ASI archaeologist of the Director's position. From the early 1980s, it had become mandatory that any archaeologist who wanted to apply for the job of an AA had to have a Diploma from the Institute along with at least an MA degree in disciplines related to archaeology, history, or epigraphy. This mandatory regulation disqualified numerous archeologists graduating from different Universities with Archaeology degrees. They would join the Institute in order to get a job in the ASI. This insistence on a Diploma from the Institute was seen as an attempt by ASI to distinguish its technique and methodology of archaeological excavation from other university departments. This was a way to standardize the ASI's archaeological practice. It was a form of disciplinarian training, which was aimed at inscribing Wheeler's method on the students and disregarding every other archaeological method. The Institute did not have its own faculty, and courses were taught by visiting faculty

[45] The Institute of Archaeology was earlier known as the School of Archaeology and was renamed as per the recommendations of the Mirdha Committee of 1984. The Lal Committee of 2001 caustically noted: "Though it has the high-sounding title of an "Institute", it is in no way a seat of higher learning or research". They recommended a total restructuring of the Institute and suggested that the Institute be a independent university which "follow the structural pattern and working system of a central university, in order to emerge as the premier archaeological and art history institution of the country comparable to similar institutions abroad" (Lal 2001: 139).

members who were invited from various Universities in the country. But the most important part of the ASI Diplomam which was a two year long course, was the two field seasons of archaeological excavation under various Excavation Branches. It was a mandatory 90-day excavation ritual, where the students were coached into the "ASI method" or the "Wheeler method". During these two years, the students not only learned how to dig the ASI way but were also socialized into the bureaucratic practice of the ASI. Over the course of time, the connection between the Institute's Diploma and the ASI had become so powerful that almost all AAs were who joined the ASI were trained in the Institute and students from other universities who even had PhDs in Archaeology were not taken in as they did not have the requisite Diploma. This "in-breeding", as an ex- student called it, was the main cause of the inability of the "ASI to learn new methods of digging and new technologies of excavation. This makes people in the ASI insecure about new theories in archaeology - processual or post-processual. They are closed an uninterested in learning".

Once the students graduated from the Institute they were not immediately absorbed by the ASI, but had to work a number of years doing odd jobs, completing PhD, waiting for the ASI to issue the "job advertisement" in the "Employment News"[46]. Talking to numerous AAs and ex-students, I came to know that this "waiting period", could range from a couple of years to five years and some of the students were never able to get a job in the ASI. In 1996, ex-students of the Institute who had been waiting for nearly six years, formed a students' Union and organized a relay hunger strike followed by a strike [*gherao*] at the DG office to demand for issuing of the AA post. This was done after numerous petitions and after official channels had been explored, and I was informed by the chief organizer of the strike (now an AA at Baror) that the "strike was the last resort. It had to be done for the livelihood of unemployed students. We had lobbied with everybody from the *babus* in the Ministry to the MPs [Member of the Parliament]. We even managed to get questions raised in the Parliament. But nothing happened. We then had to *gherao* the DG and court-arrest ourselves. This gave us a lot of press coverage and the next day I was given an audience with the Home Minister L.K. Advani and soon after that, more than thirty posts were released. But it took more than a year from the

[46] The educational Qualification for the post of AA read as follows: " Essential : Candidates other than SC/ST category should have secured at least 55% marks (without grace marks) in master's Degree (or)equivalent in Ancient Indian History / Archaeology / Sanskrit / Persian / Prakit / Pali / Arabic / Anthropology / Geology of any recognized university or equivalent. SC/ST category candidates should have secured at least 50% marks (without grace marks) in Master's Degree in these subjects. Desirable: Knowledge of Stone Age Archaeology / Art and Architecture" (Lal 2001: 126).

post being advertised to jobs being awarded. But the majority of those students who participated in the strike got the job".

I was told that it had become standard practice for students to wait for a considerable period of time before they could apply for a job at the ASI. When I started fieldwork, while talking to the students and the ex-students, I could sense an excitement, as there was a rumor that the ASI would soon be advertising for fifty-five posts of AA. By the time I completed my fieldwork, half the number of jobs had been announced and some of the ex-students I had known had become AAs. I was told that these posts were released because Jagmohan, who was then the Minister, had pushed through the upper echelons of the decision-making body at the Cabinet level to get these new posts released. "Or we would have had to also do some kind of an agitation" remarked an ex-student who would scan every week the copy of Employment News that he would buy from the local district headquarters nearly a hundred kilometers away from the excavation site looking for the job advertisement for AA.

Conclusion

Thus we see that the ASI as a postcolonial organization was a deeply hierarchical one, which had a powerful impact on the subjectivity of each individual employee of the organization. Each level of the hierarchy had grievances and allocated agency of its misery to agents outside its cadre. This had an important impact on the way excavation work was organized, as we shall see in the subsequent chapters.

Chapter 3

Spatial formation of the Archaeological Field

The Archaeological Field

The idea of the *field* and the practice of *fieldwork* are crucial to the self-imagination of the archaeologist as a scientist and central in archaeology's claim to its epistemological status as a scientific discipline. The field is conceived of as an epistemic site by most theoretical archaeologists – often described as the location for knowledge production analogous to a laboratory for the physical scientist (Lucas 2001:10-14). Archaeologists frame the field as a discursive location crucial to the disciplinary formation of archaeology to distinguish itself from laboratory sciences because of the methodological intervention it conducts to produce knowledge. As the principal site for knowledge production, the field is where knowledge is discovered, generated, and constructed within the disciplinary discourse of archaeology.

The idea of the field as an epistemological category has its genesis in late nineteenth century archaeology alongside other field sciences such as botany, geology, and anthropology. Venturing into the field to amass knowledge was a well-known practice in the antiquarian traditions of the seventeenth and eighteenth-century European scholars (Schnapp 1996: 182-219). In the colonial setting, the field was genealogically tied to the emergence of colonial explorers and travelers, whose exploits into unknown territories were aimed to produce knowledge of the other (Pratt: 1992). The field was conceptualized as an *instrumental engagement* with a landscape - necessary in order to gain knowledge about material objects. The important distinction between antiquarian fieldwork and scientific fieldwork was that the former focused on *collection of antiquities* whereas the latter involved a transformation of the landscape though excavation to *produce scientific data.* Antiquarian fieldwork was an *observational engagement* with the landscape - elements on the surface of the landscape were studied, categorized, and interpreted to create a meaning for the past. Scientific fieldwork, as it evolved in the late nineteenth century, was an *interventionist engagement* with the landscape, - involving not just observational techniques but a practice of transforming the landscape using systematic methodology. Central to this interventionist practice was the theory of excavation, originating from the belief that uncovering the landscape would lead to production of scientific knowledge. As a result, the archaeological field was conceptualized as epistemological practice, this conception being underscored and reiterated through more than

a century of writing on archaeological theory and practice.

The field in archaeology is understood as a physical space, outside of and clearly demarcated from the domain of domestic space inhabited by the archaeologist in an academic or professional setting. The field is the *place* where an archaeologist goes in order to generate data about the past. The field is the physical landscape on which methodical scientific intervention produces knowledge about the past. Sir Leonard Woolley articulates in his 1930's textbook on archaeological methods and fieldwork, *Digging up the Past*: "Field Archaeology is the application of scientific method to the excavation of ancient objects, and it is based on the theory that the historical value of an object depends not so much on the nature of the object itself as on its associations, which only scientific excavation can detect" (Wooley 1930: 15-16). For Woolley, who spent a lifetime digging in the *Orient*, field archaeology was a practice that involved transforming an archaeologically potent landscape into an epistemological space with the aim "to discover and to illustrate the course of human history" (Wooley 1930:31). Philip Barker (1977), writing in the *Techniques of Archaeological Excavation* conceives of the archaeological field as a landscape, "which is a vast historical document. On its surface has accumulated a continuous accretion of hundreds of thousands of small acts of change, both natural and human" (Barker 1977:11). The field is a potent landscape that requires intervention of the archaeological science in order to exploit its epistemological potency. The archaeological field is thus seen as a spatial entity, *charged* with epistemological meaning, waiting to be discovered by the archaeologist who is willing to leave the confines of his armchair and venture into this potent landscape to discover past.

I argue in this chapter that the *field* in archaeology not only evokes the epistemological notion of the landscape, but it also relates to a *social engagement* with the landscape. It is not only an area where archaeologists physically situate themselves to perform science, but rather it is a discursive space bounded within the confines of a specific cultural sphere and social system. Early archaeologists like Flinders Petrie, Leonard Woolley, and Mortimer Wheeler talk about the idea of fieldwork in archaeology as involving social intervention in landscape (see Petrie 1904; Woolley 1930; Wheeler 1954). However, later articulators of archaeological theory and practice seem to negate the idea of the field as a social category and celebrate the field and the practice of fieldwork as being predominantly an epistemological location and a scientifically driven practice respectively.

This chapter destabilizes the meaning of archaeological fieldwork defined as an epistemological sphere, by theoreticians and fieldworkers alike (Petrie 1904; Wheeler 1958; Wooley 1930; Clark 1968; Renfrew & Bahn 1992; Hodder 1999; Lucas 2001), and demonstrates that archaeological fieldwork is essentially an ideological engagement with landscape and a manipulation of the landscape's spatiality to assemble an epistemic domain conducive for the production of archaeological knowledge. I argue that fieldwork involves more than an epistemological *field*. It constructs a socio-ideological *field*; archaeological knowledge is not simply the product of an unmediated practice of knowledge production, but emerges from multifaceted cultural and political engagements with landscape, materiality, people and social networks. Specifically, in this chapter, I critically examine the spatial formation of the archaeological field and the daily practices through which the ASI, as a statist organization, engages with the landscape and transforms it into an epistemic location. Through ethnographic description, I establish that the ASI archaeological excavation is essentially a *(post)colonial exploration project* – a genre of colonial science which emphasizes that the "real" process of knowledge production is situated outside the domains of the metropole at the fringes of the empire (nation). I show that, as a postcolonial statist organization, the ASI's conceptualization of the archaeological project is ordered by an ideological theory of land and landscape. It is constructed within the colonial framework of territory, and structured by the fundamental ideas of *discovery*, *occupation,* and *colonization*. The focus of this chapter will only be on the first intervention in an excavation project - that of discovering the archaeological site, acquiring it, and setting up of the archaeological camp. In this chapter I investigate the archaeological field not just as an epistemological act but as an ideological intervention in the landscape as a social and cultural entity. Through ethnographic observation and experiences, I show that archaeological intervention of the landscape by the ASI is fundamentally a social intrusion, justified as an epistemological project. The three stages of intervention that I examine, demonstrate the political and the ideological manifestation of the archaeological field. The first stage, consisting of the moment of discovery, raises issues concerning the politics involved in the discovery of an archaeological site and the attribution of *credit for discovery*. I show that the discovery of an archaeological site for the ASI, although framed as part of a research program, is primarily a political project of claiming proprietary rights over the landscape, by *recognizing* a piece of land to be epistemologically potent. The process of acquiring the epistemologically significant landscape constitutes the

second stage. Through the invocation of a battery of legal directives and injunctions, the ASI, in an inherently unjust way, acquires the landscape. Finally, the third stage is one by which the ASI transforms the acquired landscape into a symbol of state power. The landscape is inscribed with the statist spatial formation of the campsite. I frame this form of ideological intervention of the landscape within the political logic of colonial exploration project. I demonstrate that archaeological fieldwork in the postcolony, although being conducted as a nationalist project, is inherently the continuation of the colonial project of exploration and expansion.

Theoretically, this chapter engages with theories of spatial practices as elaborated in the works of Henri Lefebvre, Michel Foucault, and Michel de Certeau. Spatial practices are broadly construed "as a field where space, ideology, and representation are joined in generative relations" (Liggett & Perry 1995: 6). Here space is not treated as "the dead, the fixed, the undialectical, the immobile" (Foucault 1977:70), but as a productive process of daily and institutional practices. Space is viewed as a continuous practice of meaning making - part of our semiotic sphere of being. This theoretical lens privileges "everyday operations", where space is constantly created through professional practices - both dominating and reductive (Certeau 1988). Space consisting of the built environment, architectural features, and material cultures is not considered to be merely a physical manifestation of human intervention, but rather these are products of ideological and symbolic processes. Thus, space is conceptualized as a social and political product and spatial practices as conjugative processes that are produced through a "complex constellation of the ideological and the material" (Liggett &Perry 1995: 9).

Fundamental to this critical theory of spatial practice is the notion of space articulated by Lefebvre as a process, which is produced "in inseparable yet shifting physical and social context" (Liggett & Perry 1995: 6). In *The Production of Space*, Lefebvre develops a unitary theory of space. Unlike Foucault, Lefebvre's work is not a local theory of space and politics but one which can apply to space across different levels, from classrooms to global structures. Lefebvre's theory of space is rooted is the long tradition of materialist theory, dedicated in conjoining the abstract and the material and bringing them to bear upon each other. Similar to Marx, who shows that commodities can be thought of as the material embodiment of labor power, Lefebvre demonstrates that space is an embodiment of social order: "There is a politics

of space because space is political" (Lefebvre 1991: 59). Lefebvre develops a schematic triad that unites physical, mental, and social space and provides us with the conceptual apparatus to analyze the production of space: spatial practice, representations of space, and representational space. Spatial practices have to do with everyday social/spatial patterns of people in particular places. Representations of space are codified and institutionalized ways of knowing space. Representational space is heavily loaded, deeply symbolic space that is not necessarily conscious. By employing this theoretical matrix, in this chapter, I want to show that the archaeological project, beyond its epistemological goals, is more fundamentally a social production of space. Lefebvre's schematic triad illuminates the various facets of this production. This chapter investigates how the ASI, simultaneously functioning as a statist organization and as an archaeological organization, transforms a landscape designated as archaeologically potent into a space conducive for the production of knowledge. By focusing on *spatial practice* as defined by Lefebvre, I examine the social and the ideological processes in the construction of a statist - archaeological complex. I demonstrate how the production of this spatial complex is an every day iterative process through which the ASI produces a hierarchical spatial formation, which is constantly generated and used.

Discovery of the Archaeological Site

The archaeological project is an extension of the colonial project of *discovery*, and for the post colonial ASI - the concept of discovery is central to the archaeological enterprise. As the Excavation Director at Hansi explained while giving me the site tour: "Mr. Chadha, archeology is nothing but discovery" [*Chadha sahab, archaeology main discovery nahi ki, toh kya kiya*]. Within the popular imagination of the ASI employees, the prestige of an archaeologist was not assessed on the basis of his/her analytical or theoretical contribution to Indian archaeology but on the number of sites discovered and the temporal antiquity of the site. It was understood that the older the site the more prestigious it was to discover and dig it. Discovery primarily meant uncovering new sites, and bringing hitherto unknown sites into the site-catalogue of Indian archaeology. The archaeological practice of discovery, in the context of the ASI did not pertain to uncovering a structure or an artifact in the trench or the quadrant, but only to identifying new archaeological landscapes, unearthing a site and thus contributing to the knowledge universe of Indian archaeology.

"But what about theory?" I asked the Excavation Director of Hansi. He condescendingly

quipped " It is you people at Deccan College who do theory, we in the ASI dig" [*Theory-wori apke Deccan College wale karthe hain, ham ASI main khudai karte hai*].[47] The predominance of discovery and digging was central to the imagination of ASI archaeologists, and theory was viewed as an effeminate indulgence of armchair archaeologists. Most ASI archaeologists believed that discovery and excavation was the primary task of ASI archaeologists. Like the Hansi Excavation Director, they considered theory and analysis to be secondary and believed that it was the task of a university archaeology department like the Deccan College. The ASI took pride in producing primary data for archaeologists who were involved with analytical and theoretical work: "if we do not produce data what will the archaeologists at Deccan College do?" [*Agar hum ne* data *nahi nikala toh aap ke Deccan College wale kaya karegnge?*]. The primacy given to discovering and digging of the excavation site reflected the ASI's conceptualization and engagement with land. The ancient landscape was there only waiting to be discovered and this discovery was a matter of personal prestige and fame.

The archaeological site was imagined as a spatial formation to be discovered and brought forth into the collective consciousness of the archaeological community in India. The narrative of discovery was often framed within nationalist terminology as, - an Assistant Archaeologist at Baror with pride explained: "For us, archaeology is not work, but worship. We are digging the nation (Indianness)" [*hamare liye arcaheology kaam nahi, puja hai, hum bharatiyata ko khod rahe hai*]. As professionals, all young Asst. Archaeologists that I spoke to wanted to discover a site of their own and report it to the archaeological community in the Annual Report of the ASI, and announce their discovery in the annual national conference on archaeology.

Discovery of new sites was institutionalized in the ASI in the form of the "*Village-to-village survey for antiquarian remains*". Shortly after joining the ASI young archaeologists would conduct this survey, which involved:

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[47] Deccan College Post Graduate & Research Institute in Pune, was one of the earliest education institute in western India established in 1851. Its genealogy was located in Lord Elphinston's 1824 report on the Hindoo College: "A class of men were maintained whose time was devoted to the cultivation of understanding of ancient texts: their learning may have been obscure and degenerate but it still bore some affinity to real science into which it might in time have improved. They were not perhaps much inferior to those monks among whom the seeds of European learning were long kept alive" (*Maharashtra Herald*, 17 Aug. 2003). In 1948, Prof. S.D. Sankalia started the first academic department of archaeology in Asia here, and since then its reputation in Indian archaeology has that been that of focusing on theory.

> ...surveying all the villages in India district-wise for bringing to light archaeological remains. This was carried out through the posting of Assistant Archaeologists in the sub-circles and they were instructed to select districts and start the exploration in a systematic manner. The entire operation was monitored at the Circle headquarters by the Superintending Archaeologist (AACD, F.No. 24/2/2003-EE).

During this operation, each discovery, however minute, had to be catalogued in an official format called the "Form – D." If a significant site was discovered, a senior ASI officer would further inspect it and then this site might be declared a protected monument. Although the idea of discovery was central to all operations of the archaeological project, as we will see in later sections, its tropological significance became overwhelming in the context of a newly discovered site. Here the discovery was analogous to the sighting of a new land, framed within the colonial logic of occupation of a terrain. In the archaeological case, the discovery was not just a colonization of landscape but it was also a spatial occupation of temporality. This previous phrase is not clear. Since the discovery of site was a "matter of prestige," I learnt that it was not free of political ramifications and machinations.

Dr. Jagat Pati Joshi officially discovered Dholavira along with one hundred and twenty sites dating from the prehistoric to the historical periods in the 1960s. These discoveries were part of a "systematic and planned exploration" work carried out by Joshi during 1964-65 and the winters of 1965 and 1968, in order to "help in the reconstruction of complete history of Kutch (Joshi 1990: 3). The ASI archaeologists at Dholavira talked about this discovery of Dholavira in hagiographic terms – Joshi, according to the "legend", came to Dholavira after a three day journey on camel back while he was excavating the site of Surkotada. However, while doing my fieldwork, I came to know that the indigenous villagers of Dholavira contested this official narrative.

In the dominant contesting narrative, Dholavira's discovery was attributed to Shambhudan Gadhvi, the upper caste ex-headman of the village.[48] According to the story narrated by him and recounted by the laborers I spoke to at the site - In the 1960s, Shambhudan Gadhvi had

[48] Gadhvi was an honorific title for a bardic caste of Gujarat called the *Charans* - a nonmendicant upper caste (however not Brahmins or Kshtriya) who exerted considerable influence on the local polity since medieval times (Shah & Shroff 1958).

discovered numerous Indus seals from the site-mound while he was a foreman at a drought relief project. While supervising the digging of a canal to collect the monsoon waters [*bandh kaam*] he discovered numerous artifacts, prominent among them was an Indus seal. He recognized that the seal belonged to the Indus civilization [*Harappan sabhayta*] by comparing it to the pictures that he found in his son's history book. He subsequently searched for more seals and collected numerous artifacts - decorative ceramics, fragments of carnelian beads, and metal objects. He took these artifacts to the district museum in Bhuj (the district head quarters of Kutch) and showed it to the curator there. During my interview with Shambhudan Gadhvi, he emphasized that he did not believe that J.P. Joshi had ever come to Dholavira, because otherwise, being the headman of the village at that time, he would have surely known about it. He believed that Joshi must have seen the artifacts that he, Gadhvi, had submitted at the Bhuj Museum and reported the site to the ASI. According to him, it was Dr. R.S. Bisht, who had been the first ASI official to visit the site in the mid-1980s. By that time, the backyard of his house was full of artifacts, ceramics and structural members from the site. Gadhvi had been an important collaborator of Dr. Bisht, while large-scale exploration work was being carried out in the region to create a settlement map of the island of Khadir where Dholavira was located. Gadhvi claimed that it was due to his contacts with people in other villages that it had been possible for the ASI to discover more sites. Once the excavation work commenced in 1990, Gadhvi had helped the ASI to set up the camp and recruit laborers from the village of Dholavira for the first season of excavation. When I asked the Co-Director of Dholavira about Gadhvi's claim, he admitted that it was true that Gadhvi had been aware of the site before ASI's intervention, and that he had indeed been very helpful during the earlier years of excavation by providing important local logistical support. "But to give him the credit for the discovery of the site would undermine our role as archaeologists," claimed an Asst. Archeologist who believed that "it was one thing to be aware of the site and another to know the archaeological value of the site - *discovery is not about awareness but about recognition.*"

However, Shambhudan Gadhvi's claim was only the dominant narrative of Dholavira discovery. Another minor narrative attributed the discovery of the site to a young goat herder boy, who while grazing his goats and sheep had found an Indus seal, and had shown it to Gadhvi, who, *recognizing* the importance of the seal, had taken it to the museum in Bhuj[49].

[49] Bhuj was the district headquarters of Kutch. The museum was established in 1877 and was located in the medieval walled city, which during the 2001 Gujarat earthquake was flattened.

When I asked Gadhvi, he admitted this to be true. However Gadhvi claimed that he still should be given the credit for the discovery, because he had been able to recognize the importance of the seal and had reported the site to the Bhuj museum. This minor narrative also reflected the caste tension between the various groups at the village. It should not come as a surprise that the boy, who was now a man from a lower caste than Gadhvi, also wanted to be credited with the discovery of site. Nonetheless, these claims did not make a dent in the official hagiography of the ASI; the painted iron board erected at the entrance of Dholavira erased the local narrative of discovery and blatantly announced: "The Harappan site at Dholavira (Lat. 23.53'.10" and Long. 70.13'.00" E) has been discovered by Shri Jagat Pati Joshi in 1967-68. The Archaeological Survey of India has started excavation for the first time in 1990, under the directions of Dr. R.S. Bisht."

The attribution of credit to a discovery did not depend on the claim to initial sighting but rather on the *idea of recognition.* Discovery did not signify the ability to locate new sites, but rather the ability to *recognize* the archaeological potential in a site. In epistemological terms, *recognition* refers the professional ability of the archaeologist to invoke the knowledge base of archaeology in India to categorize the site as archaeologically valuable. For Shambhudan Gadhvi, the shepherd boy did not deserve the credit for discovering the site because he had been unable to recognize its antiquity. For the ASI archaeologists, Shambhudan Gadhvi was not worthy of being credited as the discoverer of Dholavira because he had been unable to recognize the epistemic value of Dholavira as an archaeological site. The ability to recognize, as I will show in Chapter 4, is fundamental to the professional practice of archaeology, which mediates the daily practice of archaeology as discovery and determines the value of each archaeologist. Thus, discovery of the archaeological site constitutes both a discursive act of recognizing the landscape as archaeologically valuable and a process through which the landscape must be colonized for epistemological exploitation. Recognizing and, crucially, exploiting the land for archaeological excavation, was the prerogative of the statist machinery. Once a site had been discovered, reported, announced and noted in the registry of the Indian archaeological catalogue of sites, the next step was to physically take over the land, occupying it in order to commence the task of unearthing the past for the nation [*bharatiyata ke liye*]. This was the final task before the archaeological landscape could be transformed into an epistemic landscape. This process of acquiring the land from the local inhabitants was a complicated one. It began with the invocation and application of colonial laws to acquire land

that did not legally belong to the state but was transferred to the state via a process of unequal negotiation – since the state had recognized the importance of that landscape for the nation.

The Legalities of "Protected Site"

In 1878, twenty years after the establishment of the ASI, the colonial government formulated the first of a series of numerous Acts and Regulations concerning the protection, governance and acquisition of ancient remains. The first of these Acts was the Treasure Trove Act of 1878, which was followed by the Ancient Monuments Preservation Act, 1904, and the Ancient Monuments Preservation Rules, 1937. After 1947, the postcolonial government, taking the Ancient Monuments Preservation Act, 1904 as their framework, formulated other Acts concerning archaeology in India. These included: the Ancient and Historical Monuments and Archaeological Sites and Remains (declaration of national importance) Act, 1951; followed by the Ancient Monuments and Archaeological Sites and Remains Act, 1958; The Ancient Monuments and Archaeological Sites and Remains Rules, 1959; Antiquities and Art Treasures Act, 1972; and Antiquities and Art Treasures Rules, 1973. These, along with Export Control Act, 1947, and the Public Premises (Eviction of Unauthorized Occupants) Act, 1971 constitute the legal structure through which the ASI protects all forms of archaeological heritage.

The notion of the "protected monument" is central to administrative control over any archaeological site by the ASI throughout India, and is defined by the Ancient Monuments and Archaeological Sites and Remains Act 1958 as "an ancient monument which is declared to be of national importance by or under this Act". An ancient monument is defined as:

"any structure, erection, or monument, or any tumulus or place of interment, or any cave, rock-sculpture, inscription or monolith, which is of historical, archaeological, or artistic interest, or any remains thereof, and includes—

- the site of an ancient monument;
- such portion of land adjoining the site of an ancient monument as may be required for fencing or covering in or otherwise preserving such monument; and
- the means of access to and convenient inspection of an ancient monument"

This Act gives unlimited power to the ASI to declare any piece of land in India that it considers *archaeologically valuable* to be declared protected: " If the Central Government is of opinion that any protected area contains an ancient monument or antiquities of national

interest and value, it may acquire such area under the provisions of the Land Acquisition Act, 1894, as if the acquisition were for a public purpose within the making of that Act." The ASI through the Ministry of Culture did this by issuing a notification in the Official Gazette, which declares an ancient monument to be a protected site. Once the site has been declared protected the lengthy process of land acquisition begins. This transference of property is usually accompanied by litigation, and the court cases might drag on for years. Elucidating the process of land acquisition, the *Archaeological Works Code* states:

> When land is required for public purposes by the Survey, the head of the Office, should, in the first instance consult the Collector / Revenue Officer of the District and obtain from him, all possible information as to the probable cost of the land, together with the value of the building, etc., situated on the property, for which compensation will have to be paid. Upon the information thus obtained an estimate should be framed by the head of the Office and submitted to the Director general for his sanction. When sanction to such an estimate has been obtained, the head of Office should communicate the matter to the revenue officer, who will take the necessary action for acquisition of the land, under the Land Acquisition Act, or its acquisition by private negotiation (Archaeological Works Code: 57-58).

Acquiring the Excavation Site

One of the most disconcerting features of the ASI excavation sites which I studied, was that after the land was acquired for excavation, proper compensation was not paid to the people on whose land the excavation was being conducted. The Ancient Monuments and Archaeological Sites and Remains Act 1958 states that "Any owner or occupier of land who has sustained any loss or damage or any diminution of profits from the land by reason of any entry on, or excavations in, such land or the exercise of any other power conferred by this Act shall be paid compensation by the Central Government for such loss, damage, or diminution of profits." During my ethnography I came to know that land was usually acquired through negotiations and most of the time, due compensation was either not given to the people at all or that it was delayed by several years. This was not an unusual occurrence in postcolonial India; coercive acquisition of land for developmental work was a norm and bureaucratic hurdles often resulted in failure in the payment of compensation. The ASI, as a governmental organization, was no different, and almost all the sites I worked in were involved in disputes relating to land acquisition and compensation.

The ASI or the State Archaeological organization had declared some of the Saraswati Heritage Project (SHP) sites as "protected monuments" soon after they were discovered during ASI exploration work. For instance, Dholavira and Juni Kuran were declared protected in the 1970s soon after J.P. Joshi *discovered* them. On the other hand, sites such as Baror, Bhirrana, Chak 86, Tarkhanwala Dera, and numerous sites that were recently included as part of the SHP, were discovered by Dr. A.K. Ghosh in the 1950s, but were only declared protected sites in 2003 after it had been decided to conduct excavation at these sites. The process of the land acquisition of these SHP sites commenced in the middle of 2003. Letters were sent to the Superintending Archaeologists of the Rajasthan, Haryana, and Gujarat Circles to begin the paper work necessary to acquire land for archaeological excavations. In the meantime, advanced teams from the Excavation Branches were sent to the sites to conduct reconnaissance trips to ascertain the nature of the mounds and to coordinate with the local district level officials to acquire at least temporary permits to conduct excavations at the sites. This was a complicated procedure and involved a number of official and non-official negotiations, which I obviously could not be privy to. However, the tension and contestation regarding proprietary rights over the land in which the excavation was being conducted, was a uniform feature of all the sites. The primary cause of this tension was the (fair) compensation being demanded by the owner(s) of the land and the ASI's inability to fulfill those demands in a favorable way. The ASI's track record in giving compensation to the people who owned the land which was required for excavation purposes, was quite bad. For example, during a visit to the Kalibangan excavation site, I was told that the people on whose land the Kalibangan Site Museum had been built in the 1980s had yet to be given compensation. Although it has been difficult to obtain statistics about the number of litigation cases over issues of land acquisition and compensation that the ASI was involved in, I was told by a number of ASI archaeologists that a lot of their time in the office was spent in preparing for cases and appearing in the court on numerous land acquisition dispute cases.

The excavation in Dholavira began in 1990 and the process of land acquisition had commenced more than fifteen years before I came to Dholavira to conduct this ethnography. I had expected that the process of land acquisition would have been completed and that people on whose land the archaeological excavation was being conducted, would have been duly compensated, but that was far from reality. Dholavira was the largest of the SHP sites and

covered an area of 120 acres. A large chunk of this land, primarily non-cultivable, belonged to the Forestry Department of the District of Kutch, whereas the rest of the land belonged to fourteen land-owners from the village of Dholavira. Out of the fourteen owners, only two owners lost all their land; the others lost part of their land. This part was cultivable land, where only one millet crop (*jowar, bajra*) could be grown a year, fed by the sparse monsoons in this very arid region. Even by the time I arrived, fifteen years later, none of the fourteen land-owners had been paid compensation by the local administration. Upon asking, I was informed that it was not in the jurisdiction of the ASI to pay compensation, but that it was the responsibility of the Revenue Department of the District of Kutch to acquire, evaluate, and pay compensation to the affected land-owners. However, I came to know from the laborers that the officials responsible for acquiring the land had not even initiated the process of valuation of the land and that the question of compensation was not even in the picture. Most of the fourteen owners belonged to the lower castes and did not have any political power to raise their voice against the ASI. The ASI archaeologists had defused the situation by employing two members of each of the land-owners' families as daily wage labors at the site, giving them Rs. 92 per day throughout the excavation season, which lasted almost 100-120 days every year. This amount was a princely sum of money for anyone in Dholavira. However, it is important to point out that this was not any form of legal compensation for the acquired land, but rather the result of an informal negotiation process that the ASI archaeologists had to get into in order to mitigate any form of protest from the land-owners which would affect the ASI excavation.

Of these fourteen owners, one was not satisfied with the informal compensation that the ASI archaeologists had given him - three members of his family were employed as daily wagers on the excavation site. Mohan *bhai* (Gujarati suffix for respect) was among the two owners who had lost all their land and, unlike the others, he was an upper caste Brahmin who had, several times, attempted to raise the banner of revolt against the paltry temporary compensation offered by the ASI. It is important to note here that it was possible for him to raise his voice because he was an upper caste Brahmin - the only one in the village of Dholavira, which mainly consisted of lower caste and tribal communities. Socially, Mohan *bhai* had considerable clout in the village, and it was no wonder that he was the only one in the village willing to challenge the statist ASI. I heard numerous stories about how he had shown his displeasure by destroying trenches on the site by digging through the quadrant walls and the

balks in the middle of the night, how he had threatened other workers, and how, at one time, he had accosted the director of the site in a drunken stupor, brandishing a sword. In one of my conversations with him, he told me that he was losing nearly 7 acres of land (6 acres and 26 *guntas*), and that he was not willing to accept the paltry, temporary compensation that the ASI was "throwing as crumbs" at him. He said that he had been very polite with the Director and other ASI archaeologists thus far, but that now he was planning to go to the court against the ASI, and would not rest until he "stopped the excavation project." He explained that this was the last resort for him. Five years ago, the Excavation Director had promised him that the ASI would pay him Rs. 2000 every season as compensation for the dirt they excavated from his land, along with the daily wage employment of three members of his family. Although the ASI had kept their word as far as the employment of his family members was concerned, they had not paid him the promised Rs. 2000 for the dirt that had been excavated in the past three years. He told me that he had also approached the Director and the Deputy Director a number of times to expedite the process of compensation, but that nothing had been done. He claimed that he had visited the Revenue Department office in Bhuj (District Head Quarter of Kutch) many times but no action had been taken. Then Mohan bhai pulled out a dirty plastic folder, which contained a paper clipping from a local Gujarati newspaper *Kutch Mitra*, in which he had written a letter to the editor explaining his complaint against the ASI and the government of India. Mohan *bhai* was illiterate but he had enlisted the help of a college student to write this letter for him. The letter read, "I was lucky that an archaeological site was discovered by the ASI in Dholavira, but this luck of mine has become a burden as the government of India had not yet paid me compensation for the land that I lost (6 acres and 26 guntas). I have worked at this site for many years as a watchman *(chowkidar)* and this site is indeed the pride (*gaurav*) of Kutch and of India, but this pride is weighing me down." Mohan bhai, towards the end of the conversation, told me that he did not really want any monetary compensation for the land that he had lost because of the excavation: "this is my gift to the nation"- but he hoped that at least his son would be given a permanent job by the ASI - as a caretaker of the site. This was a surprising understanding of the situation, for it revealed that Mohan bhai had realized that his land had been permanently taken away from him and that the compensation would only provide temporary respite. By demanding a permanent job for his son he wanted to strike a more stable and long-term bargain. Mohan bhai had realized that Dholavira would become an important tourist attraction, and a salaried income would be a more viable than a one-time monetary recompense.

At Hansi, Baror, and Bhirrana, unlike Dholavira, excavation began in the winter of 2003, and I had the chance to be present during the first season of the excavation and was able to observe how ASI archaeologists negotiate with local landowners in the process of acquiring land. At Hansi the problem of land acquisition had been solved several years ago. The Hansi site was a monumental mound enclosed within a medieval fort, surrounded by the town of Hansi. The mound had been declared a protected monument in 1962. The problem at Hansi was not of land acquisition but of the encroachment of the town over the mound, which had destroyed the fringes of the mound. The ASI archaeologists on site were not very concerned with this destruction as the site was very rich and they were laying trenches and digging at the top of the mound. The Asst. Archaeologist told me that they had reported to the local district administration about the encroachment many months before they had started digging at the site, but nothing had been done. He believed that it might just be impossible to get the encroachers to move away from the site since they had been living there for almost a decade. He suspected that this might be the reason that the local district administration was not keen to get involved with the eviction of the encroachers.

At Bhirrana and Baror, the situation was very different. Both of these were small mounds with habitational deposits of around 6-8 meters. They had been declared protected monuments in the 1960s, soon after A.K. Ghosh reported them after his exploration of this region of western India in the 1950s. Unlike other sites, a serious problem persisted in these mounds: they were regularly utilized as Muslim burial grounds. During the pre-partition years, there was a substantial Muslim population in this part of India. After independence, the proprietary rights of these mounds were given to the State Wakf Board - a semi autonomous, governmental Muslim body, which managed religious and community property. Until 2004, the local Muslim community members buried their dead on these mounds - although the frequency of such burials was low - less than a dozen a year. In Bhirrana, the Director of the excavation had begun administrative paperwork for transfer of the land a few months before the excavation began. By the time the campsite was set up in December of 2003, permission had been acquired by the local district administration to allow excavation at the site. Since the Wakf Board was part of the state administration, no monetary transaction took place, as a matter of fact, strict orders were issued by the Wakf Board to prohibit any form of burial at these sites.

At Baror, a similar situation existed: this mound was also the burial ground for the local Muslim community, and the land belonged to the Rajasthan Wakf Board. The site had been declared a protected monument in 1962 but the Wakf Board continued to have proprietary control over the land and used the mound as a burial ground. The fringes of the mound were also used as a burning place for the Hindu and the Sikh populations in the area. On the top-most portion of the mound was a brick and mortar shrine dedicated to a local Muslim saint, who was buried there. The erection of sacred shrines over archaeological mounds has been a regular practice in various parts of western India. According to the local villagers, this was a very recent phenomenon- and most of the shrines are not more than 20-30 years old. However, in the case of Baror, the presence of the shrine proved to be a big logistical problem for the ASI. Soon after the Patna Excavation Branch had pitched their camp at the site, the local Wakf Board members along with community leaders, lodged a formal protest against the ASI and requested that the excavation be stopped because it violated their sacred space. The Board simultaneously demarcated the whole mound with barbed wire to prevent the ASI from conducting excavation, and even threatened the labors at the site with dire consequences if they worked for the ASI. The Sub-Divisional Magistrate (SDM) of Anoopgad District was immediately informed. He came with the police because the situation was tense and there was the possibility of the situation escalating into a communal confrontation. He advised both the Muslim community leaders and the ASI to come to a compromise rather than get into a confrontation. He informed the Wakf Board members that the ASI had the legal right to take over the mound as they had the "*Act* in their favor." As for the ASI archaeologists who were negotiating with the community, he warned them that although they could legally continue with excavation "it was important to work with *tact*". He emphasized that it was more productive to "use tact rather than the legal *act*". Eventually, following the advise of the SDM, both parties reached an agreement; it was decided that the ASI archaeologists would leave the area at the top of the mound, where the shrine was located, untouched, and also not dig in the western part of the mound where the most recent burials had taken place. This compromise was acceptable to both sides and the excavation commenced soon after. However, later I was told by an Asst. Archaeologist who had been involved in the negotiation, that this pact had been just a temporary solution; in the next season, the ASI planned to conduct excavation in the area that had been mutually decided as prohibited. And sure enough, in the next season (2004-05) the area considered out-of-bounds for the ASI was also excavated - however the

shrine and the area around it were left untouched.

The examples above were not isolated instances of the complications - logistical and otherwise – involved in the process of land acquisition by the ASI, but a broader illustration of how the ASI, as a statist institution, perceived its relationship to the *landscape.* For the ASI, which works within the logic of a bureaucratic organization, land was a form of commodity to which access had to be gained through transference of proprietary rights. The ASI's conception of land was no different from other statist organizations that acquired land for development purposes - as territory that was procured for a national/nation-building task. Here the epistemic relationship of the ASI with the land was of much less consequence than the relationship assumed via proprietary rights. The land had an instrumental value, deriving from the belief that archaeological work constituted service for the nation and that proprietary rights over land were rightfully securable through legal infrastructure.

The State in the camp

While in the field, the day-to-day routine of the ASI archaeologists revolves around two distinct categories of spatial organization - the excavation site and the campsite. The interaction between these two spaces involves the movement of human and material culture; and the existence of one is dependent on the other. Each of these distinct entities is defined by a definite set of social activities and mores, and limited by its spatial boundaries. The daily life of all the members of the archaeological project involves a negotiation between these two sites. Each of these sites contains multiple zones of activity with specific outcomes, and neither of them allows for the possibility of a liminal space. The excavation site is the domain where the primary knowledge production process is executed. The campsite is ostensibly the site of rest and the setting for secondary knowledge production processes, but it also plays a very important role in the social life of the archaeologists.

The camp is the central nerve of the archaeological excavation project where the action plans are drawn; the excavation site is merely the location where these plans are executed. The camp is, bureaucratically speaking, the headquarters, and the center of power in the field. In the daily functioning of the ASI, it is here that the performance of the bureaucratic state is enacted. Unlike the excavation site where dominance is unleashed over the landscape, the campsite is the bureaucratic location of archaeology as a statist enterprise - it is here that

critical administrative, financial, and planning decisions, which have crucial ramifications on the workings of the excavation site, are taken. The excavation site, being predominantly of epistemic significance, is limiting to the ASI's statist intervention. The campsite, on the other hand, offers the perfect location for the enactment of the bureaucratic state. This can be seen in the ideological impetus of the idea of the camp, its spatial organization, and in the nature of activities that are conducted around it.

Domesticating the wilderness

The camp was the official site of the bureaucratic state in the field. It was the mimesis of the *office* that the archaeologist bureaucrat occupied in the urban metropole - referred to importantly as the *Head Office*. These were usually the Excavation Branch (Ex. Br.) offices (Baror – Patna Ex. Br.; Juni Kuran –Baroda Ex. Br.; Hansi- Purana Killa, New Delhi Ex. Br.; Bhirrana- Nagpur Ex. Br.) or in the case of Dholavira, the Director-General - DG office in New Delhi. The idea of the *head office* has been firmly encapsulated within the general ideology of the bureaucratic operation of the postcolonial state. For the archaeologists, the *head office* was the centre of bureaucratic authority through which flowed the power that affected and transformed the field. Similarly, within the field, it was the camp that embodied the power centre of the archaeological intervention of ASI. It is important to note that the ASI camp cannot be conflated with the category of the 'camp site' or the 'base camp' as often described in literature on scientific exploration. The ASI camp does have its origin in the 'camp site' of an exploration party, similar to those mentioned in accounts of eighteenth or nineteenth century explorations of the colonial world. However, with the birth of the ASI in 1858, this innocuous entity was transformed into an instrument of the colonial state. In its early years, the campsite was more of a fortified temporary settlement, displaying several powerful paraphernalia of colonial authority- officers on horseback, survey instruments, and armed soldiers.

All the Harappan culture sites that I worked in were situated in arid or semi-arid terrain, usually 2-5 km away from contemporary settlements. These sites were frequently located in what was classified by the middle class ASI staff as *jungli,* wild or inhospitable, areas. In my conversation with informants, this term was very often used to describe the terrain where archaeological projects were located, even by some of the ASI staff who themselves probably originated from similarly remote villages, but in other parts of the country (Bihar, U.P., or

Maharashtra). The category of *jungli* was also subsumed under the context of a developmental idiom – *backward*. In most cases, the English word was used, but occasionally the Hindi term *pichada* was also employed to refer to the prevailing economic and the social situation in the area. Both these terms was used interchangeably and generally used to describe the surroundings that the ASI staff inhabited during the excavation. Both these terms signaled the clear line of distinction that the archaeological team drew between themselves and the space that they occupied. It is remarkable that some members of the teams who had lived on the site for 3-4 months every year for a decade, as in the case of the archaeological team at Dholavira, still framed their interaction with their landscape in these terms.

It was the abstract amalgamation of these two terms - *jungli* and *pichada* that conceptually defined the interaction of the personnel of ASI with the physical landscape they occupied. Their conception of this interaction was captured in the usage of the Hindi phrase "we have transformed this place from a jungle into a civilized place" [*hum ne toh yahan jungle mein mangal kar diya hai*]. I heard this phrase for the first time in Dholavira, but the ASI archaeologists at other sites also used it often. The etymology of the word *mangal* is the Sanskrit *mangal* - meaning auspicious. This word not only rhymes well with *jungle(i)* but also presents the contrastive *before-and-after picture* of how the presence and intervention of the ASI machinery has made auspicious the wild and hostile terrain of the excavation site. This phrase directly connotes the trope of domestication, the taming of the wild. On the ASI's perspective, the wild is not only domesticated, but the landscape is transformed to such an extent that it is sanitized of negative elements (made auspicious) and in the process made favorable for hospitable habitation. The use of the term *mangal* is significant because the ASI's act of taming of the landscape, although essentially a colonial occupation, is justified within the logic of Hindu religiosity, and acquits the domestication of its inherent violence and illegitimacy. The domestication was not viewed as a violent act but rather as a patronizing one; however, underlying this domestication narrative is the subtext of fear and dread of the undomesticated, a fear made apparent in the architecture of the campsite, as I will describe later. It took the performance of a single ritual for a space to be made 'officially' *mangal*. The setting up of any ASI camp was preceded by a rudimentary[50] *bhoomi pujan* (worshipping the earth), which involved the breaking of a coconut, and the lighting of incense. The Director of

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[50] The level of sophistication of the ritual would depend on the religious propensity and expertise of the individual excavation director.

the excavation, usually the Superintendent Archaeologist (SA) of the Excavation Branch, conducted this ritual in most cases. However, sometimes a higher officer, like the DG of the ASI, might officiate as the 'chief guest' of such a ceremony. At times, such an event would also be accompanied by the 'opening ceremony' of the excavation season.

At Juni Kuran, I had the privilege of observing this ceremony, undertaken by the Chief Guest: Additional Director General (ADG) of the ASI, who had *additional charge* as the Director of the Excavation and Exploration department (under which the Baroda Excavation Branch fell). This gentleman was also the Director of the Saraswati Heritage Project (under which the excavation at Juni Kuran was being conducted). In the center of a newly established campsite, on a freshly made mud floor still smelling of the cow dung with which it had been plastered, stood a plastic table and three molded plastic chairs arranged at one end. On these sat the ADG, the Director of Juni Kuran Excavation (who was also the Superintendent Archaeologists of the Baroda Excavation Branch), and the co-Director of the Dholavira excavation. In the company of the staff and students, the ADG was given a bouquet of flowers by the excavation Director. After cursory words of thanks, and preliminary remarks on the importance of the Juni Kuran site in the larger context of the Saraswati Heritage Project, the ADG proceeded to break the coconut on a flat slab of rock especially brought for the purpose, and then with a few quick swings of a pickaxe, he dug the earth to mark the symbolic beginning of the Juni Kuran excavation. This rather simple ceremony was simultaneously a symbolic act of domesticating the wild and a reiteration of the official hierarchies that govern the daily practice of ASI. Through the metaphorical sacrifice of the coconut (which has long stood in for the human head), the wild was placated; the act was performed as a secular statist ritual devoid of its religious context but still retaining its symbolic implication. On the other hand, the ceremonial excavation by the ADG with a pickaxe unambiguously suggested that even in the wild field, official hierarchies could not be violated. The symbolic excavation served to re-inscribe the ADG's authority and rights over the knowledge produced at the site. Within the semiotic logic of the ceremony, it was ironical that this statement of authority could only be made explicit by appropriating the most menial task of an archaeological excavation, the exclusive domain of the lowest paid member of the archaeological excavation - the daily wage laborer.

It was from the Co-Director at Dholavira, that I heard this phrase "*hum ne toh yahan jungle mein mangal kar diya hai*" [we have transformed this place from a jungle into a civilized

place] for the first time soon after I came to the campsite. It was uttered in the context of the tour that I was given of the camp by the co-Director. I was informed that I would not have any problem (*dikkath*) at the site as, "we at the ASI are experts (*maahir*) at transforming wild inhospitable terrain into livable space". Although he admitted that the ASI could not provide all the luxury and amenities (*suvidha*) that I must be used to in America, he empHansized that the campsite of Dholavira was the best (*sabse badiya*) ASI camp I would ever visit during my fieldwork. He boasted that this was the only camp which had taps with running water in the tent, in addition to other luxuries such as the constant hot water supply, and a permanent toilet (*pucca latrine*), also with a tap. Over the course of my time at Dholavira, the Co-Director often used this phrase with a sense of pride and fulfillment, whenever we walked across the camp, or while he gave guided tours to important tourists (*VIPs*) at the site. I heard this phrase at other sites too, used in the context of the camp when contrasting its organized and planned contours with the unruly landscape in which the site was located. The camps, like the excavation sites, personified the organizational ability of the ASI excavation team and signified its ability to domesticate the wild *field* in which they were forced to work. It epitomized an absolute form of state control over the landscape.

I have framed the ASI's camp within a narrative of domestication for a reason; the postcolonial Indian archaeologists saw themselves as scientists who literally penetrated the *heart of darkness*. Because survival in the wilderness was paramount - they domesticated its hostility. The usage of the colonial *heart of darkness* trope is conscious because I want to emphasize the colonial impetus of this postcolonial exercise. Although the area that the camp was located in was within the political boundary of the Indian state, the ASI, as a statist enterprise, saw the act of excavation and setting of the camp as an invasion into the wild. This is evidenced in the way that the archaeologists at Dholavira framed the genealogy of the site within the logic of this colonial trope.

The camp location

The topographic settings in which the excavation sites were located were diverse, but the camp sites associated with each of these sites had one feature in common: they were always located at some distance away from the local village settlements; enough to be separated from these. The distance of the campsite from the neighboring villages varied - in some cases, one could walk to the village, while in others, a jeep ride separated the campsite from the nearest

village. At Hansi, in particular, the effort at maintaining the distance from local settlements was obvious because the excavation site itself was in a medieval fortress right in the centre of the town. The camp, on the other hand, was located at the very periphery of the town, in the cricket-ground of an Intermediate College campus. The camp was not too far from the site, but its location demonstrated the conscious attempt at locating campsites at a physical distance from the populated rural town.[51]. It is worth noting that the geographic relationship of the campsite with the local habitation seemed to be modeled after the colonial military cantonments spread throughout India.

Cantonments have been defined as "a condition in which personnel is housed in temporary structures especially erected for shelter of troops, a collection of these structures, specially in India, a permanent military station or town, often connected with a native town or city" (Jacob 1994:2). Cantonments were a temporary military habitation that emerged with the expansion of British power in the late eighteenth century. The earliest such cantonments were established between 1765-1800's (Jacob 1994: 20). By 1857, cantonments were an important institution of the Imperial military in India. A crucial feature of these cantonment settlements was that they maintained an explicit spatial distance between themselves and the native habitation. Oldenburg, describing the new Lucknow cantonment established after 1857, writes: "the spatial arrangements in the cantonment were the antithesis of those in the old city and typical of the genre of colonial building" (Oldenburg 1989: 52). The primary motive of these cantonments were to "create a small European cosmos at the edge of the city not only to compensate the officers for the hardship of serving their country in an alien land but also to provide European solders with adequate recreational facilities so that they would be less tempted to taste the pleasures the city had to offer" (Oldenburg 1989: 53). These were deliberately established outside of the native settlements, determined by the characteristic militaristic need of keeping distance between the military and the civilians, and furthermore to emphasize the exclusivity of the military enterprise. The geographic location of the ASI campsite seemed to be motivated by an analogous ideology and the camp was always located at a noticeable distance away from the local villages. Almost all the ASI archaeologists I spoke with had underscored that it was important to have the campsite away [*door*] or separated [*alag*] from the village settlement in order to maintain the required social distance.

[51] It is important to note that the distance that I am referring is only physical for the interaction between the individuals between the camp and the local people was both intimate and laden with intricacies, as I will describe later.

According to them, the reason it was important to keep a distance from the villages was because the labor originated from there. They argued that in order to keep the social hierarchy intact, it was important to not "mix with them" [*mel-jol*]. Furthermore, they thought it necessary to maintain a distance from the murky local politics and its complicated machinations, because as a governmental agency, it was imperative to be *independent*, *neutral*, and *non-interfering* (these specific English words were used). Each of these three terms can be framed within the detached objectivity of statist ideology, which emphasizes impartial treatment of its subjects. But, ironically, embedded within this ideology was the state's desire to create an identity distinctly different from that of its subjects. This drawing of distinctions was not dissimilar to the militaristic pattern of maintaining a distance between the state and the civilians, while citing the logic of governmentality as the rationale for the enactment of this distinction. While the archaeological activity that the ASI conducted was often framed within a narrative of service to the nation and its people, the location of its campsites away from local habitation, in fact, seemed to create a disjuncture between the state and the people it served. The distancing of the camp emphasized its officiality and authority. Moreover, the extraordinary nature of the activity, which the ASI engaged in at that location, accentuated the fact that only the state apparatus, and not its ordinary local subjects, could create this form of special knowledge. This ideological subtext of physical, social, and intellectual distance was iterative and persistently reproduced and reflected in all aspects of ASI's functioning, as we shall see below.

The ASI archaeologists preferred that the camp be located at close proximity to the excavation site so that there was maximally [*jyada se jyada*] a ten-minute walk to the excavation site from the camp. In all the sites that I visited, this was the case, except *Juni Kuran*, where the campsite was about seven kilometers away from the excavation site. The proximity of the campsite and excavation was considered important as it permitted the staff to oversee the excavation closely even while they were involved with administrative work at the camp. This proximity also allowed the ASI staff to take more frequent breaks during the day and gave them sufficient time to rest in the afternoons. This last mentioned benefit was so crucial that my informants at *Juni Kuran* complained as much about having to take their afternoon nap under the sparse shade of desert vegetation, as about their movement to and from the site being dependent on the mercy of the *jeep drivers*. At times, when the jeep was not at the site, the ASI staff would have to walk fourteen kilometers back and forth between the camp and the

site. This was a key source of resentment for some members of the ASI staff at Juni Kuran, because they felt they were losing their *free-time,* and were forced to stay at the excavation site throughout the day. This, according to them, was an unusual problem, since it was customary for ASI campsites to be as close as possible to the excavation as it was *convenient* and easier (*aasan*). The Juni Kuran excavation director seemed to be aware of the problem of the abnormally great distance between the campsite and the excavation site. But there was a reason for the location of the campsite. There was a dried seasonal stream covered with gravel between the main road and the site that did not allow heavy vehicles (e.g. trucks, construction vehicles) to pass, making it difficult to erect a campsite near the excavation. My informants told me that the Director was anxious to reduce the discontent of her team and had asked her junior archaeologists and staff to explore around the campsite to see if they might find some subsidiary site close by. Eventually they did find a late Harappan site near the camp, and decided to dig it, although it was not part of the project for that year, only because of its proximity to the campsite. Thus, it seems that it was psychologically important to have the excavation site close to the camp irrespective of the archaeological merit of excavating such a site in the context of the project at hand.

There were many other reasons for situating the camp away from the village, and one of them, as one of my informants noted, was that the possibility of *making money* was low if the camp was situated in the village and utilized the available local infrastructure. Let me briefly qualify the term *making money* here. The term relates to the *narrative of corruption* that I was inundated with during my fieldwork. The opportunity for laundering money was much higher in setting up the infrastructure for a new campsite, than in hiring accommodation and office spaces for the excavation team to stay in the local village. For example, informants told me that if the archaeological camp was set up in the village, the local infrastructure like a school or the local government office (*panchayat office*) had to be utilized for accommodation etc. This did not allow the team to make large purchases of the paraphernalia required to set up the campsite. Upon asking how money was made while setting up the camp I was informed that this was done through preparation of inflated or false receipts while acquiring equipment and supplies for the camp. For example, only ten bed sheets would be bought and the local merchant would be asked to prepare a false receipt for twenty items. In another instance, receipts for bed sheets were prepared at the rate of Rs. 500 each whereas the ASI team paid the merchants only Rs.300 per bed sheet. I heard such *stories of corruption* in innumerable

conversations with my informants throughout my fieldwork. Setting up camp for every excavation required a huge amount of purchases, which included tents, iron-cots, blankets, pillows, bed sheets, coir carpets, fans, generators, kitchen equipment, tables, chairs, ladders, excavation equipments, tools, and innumerable such things. Many of these would have been unnecessary if the camp was relying on local resources in the village, but this would simultaneously decrease the possibility of *making money*. Thus within this *logic of (economic) corruption* it made much more sense to establish a completely new infrastructure for the camp at every excavation, than to make use of available local resources.

The location of the camp can be explained by the bureaucratic logic of the postcolonial state, which privileged its own exclusivity by locating the camp away from the local habitation in order to maintain both a physical and an ideological distance. As I have shown, this parallels the pattern of the colonial cantonment and the distinction between the natives and the masters that was enforced in such a camp. The ASI camp in postcolonial India employed similar iterative semiotics of colonial difference to emphasize the distinction between the representatives of statist machinery and the subaltern villagers. This distinction was further accentuated in the architectural design of the camp. The camp was not only constructed outside the space of the local village but, further, the design of the camp prevented the free interaction between the ASI team and the local people.

The architecture of the camp

The camp was the extension of the office in the field - a frontier outpost of the state on the fringes of the nation. If the *office* in the postcolonial metropole was the central unit through which the power and control flowed in the postcolony, the camp was its mimetic manifestation at the borders. The archaeological camp was not an instantiation of the statist office, but rather the bureaucratic office in the periphery of the postcolony. The camp was fashioned as the center of bureaucratic state power in the rural country. Its design and architecture attempted to mimic the offices of the bureaucratic center with which it shared a statist genealogy. The campsite, as a manifestation of the official machinery, was not just the product of the field practice of the ASI, but rather it was an outcome of temporary governmental intervention in the country. The campsite was different from other permanent governmental edifices found throughout the rural countryside - primary school buildings, health care centers, and *panchayat* offices. Unlike these permanent statist edifices, the campsite was a transient spatial

formation, which accompanied any form of provisional intervention of the state in the rural countryside, such as that involved with the development of infrastructure - roads, bridges, dams, factories, and industrial centers.

The choice of campsite was largely dependent on the closest piece of flat land available nearest to the excavation site. Similar to the excavation site, this land did not belong to the state - it was usually the property of a local villager. Since the combined area of the excavation site and the campsite was large, there were multiple owners to the land. This was often the case when the landholdings per individual were small (between 2-15 acres), and the sites were large (more than a hundred acres in the case of Dholavira). In the case of the excavation site, the ASI preferred complete transfer of the ownership rights from the landowner to the state, so as to have absolute control over the excavation area. The acquisition of the campsite – principally a temporary occupation site -- was also a product of complex negotiations. The core issue was the nature and the amount of compensation. Since the campsite was a only temporary site, the ASI could not unleash the peculiar combination of legal and judicial arsenal to acquire the area from the owner, unlike with the excavation site. Officially, according to rule 11.4.3 of the *Archaeological Work Code,* a very precise set of procedures is laid out for such a negotiation:

> When privately owned land is temporarily required for exploration or excavation works and it is proposed to fill up the trenches later on, the Head of the Office may settle with the approval of the Director General the amount of crop compensation, rent of land etc., with the owner of the land with the help of revenue authorities (Archaeological Work Code: 1979: 59)

According to my informants, this procedure of land acquisition was very rarely followed. Instead, the ASI officials often negotiated with the owner before the excavation season commenced and, depending on the social and economic status of the owner(s), they devised ingenious forms of compensation. In Baror, where the landowner was a wealthy Sikh *Jat*,[52] who would have been insulted if offered the same contract (daily wage labor) as the small and lower caste farmers of Dholavira, a different agreement was negotiated. He was given the annual right to provide security [*chowkidari theka*] to the on-site storehouse, built on his land,

[52] Jats were upper caste wealthy landowning caste in north India, and he had around 120 acres of land and also owned a couple of vehicles compared to the laborers working on the site who were either landless of had small land holdings

which amounted to a substantial sum of money.[53] The ASI practice of negotiations with local populations will be discussed in greater detail in the next chapter. Here it is sufficient to say that the above example clearly shows how the state was a malleable and flexible system, markedly different from the official *rule and codes* that were supposed to realize its ideals; in its daily practice the state recognized caste, wealth, and power and negotiated as appropriate to fulfill its desired goal.

The architecture of the camp was designed in such a way so as to further emphasize the distinction between the official statist domain and the local villagers. At the outset, there was a clear separation between the inner and the outer domain of the camp - delineated by a bamboo and rope fence that ran all around the camp, covering an area between 2 and 5 acres. The fence was weak and porous and did not provide a real barrier against any form of human or animal intrusion. Its erection basically had symbolic valence as it demarcated the state-owned [*sarkari*] area and inscribed on the landscape the state's power. However, at times the fence did have a utilitarian value, as in the case of Baror, where a barbed wire fence substituted the bamboo and rope contraption, after stray dogs entered the camp and attacked a student and an ASI *jeep-driver*. Each camp had a main entrance, which was manned twenty-four hours a day by a watchman/sentry [*chowkidar*], armed usually with a bamboo stick [*lathi*]. The watchman was not an ASI staff member but a daily wage laborer, and he earned the same wages as the laborers at the excavation site, but he enjoyed a special status. In most cases, he was a young, well built man whose demeanor represented that of a hotel bouncer in any western city club. Similar to the fence, the gate also performed more of a symbolic function than a physically preventive one, since it was constructed out of a single bamboo pole. The task of the sentry was to remove the bamboo pole to allow vehicles to enter the camp and chiefly to prohibit unemployed laborers from entering into the premises to plead for work. Adjacent to the bamboo pole gate was a plank, which announced the name of the camp. At Hansi, Baror, and Bhirrana, the plank also stated that entry into the camp without permission was not allowed. This information was written both in English and Hindi.

The signboard, the gate, the barbed wire fence and the sentries guarding the gate undoubtedly gave a very definite official aura to the camp. But the most distinct architectural feature that

[53] Rs. 92/day for 365 days a year, amounting to a princely Rs. 33,850, compared to Rs. 8000 (Rs. at Rs. 80/day for about 100 days a year), for the Dholavira farmers.

corroborated this authoritative impression, were the white tents pitched throughout the camp landscape. These were large, waterproof, canvas tents of varying sizes, erected in an organized fashion on the camp's landscape. The ASI had used such tents, according to my informants, for more than a century, and their designs have remained unchanged, as is evident from turn of the century photographs of ASI excavations. The tents, along with other architectural paraphernalia of the ASI camp, did not spatially resemble a typical statist building, like a school, or a primary health centre; its architectural materiality bore close resemblance to the military camp, which heightened the effect of official state power. The tents did not perfectly resemble those in the nearby military camp, but were iconic enough to evoke a similar awe of inspired by official power.

The Harappan culture sites were largely located on the border zone of India and Pakistan[54], and a ubiquitous feature dotting these landscapes of western India were the Indian Army and the BSF (Border Security Force) camps[55]. Similar to the ASI camps, these were closed spatial establishments, more heavily fortified (with multiple layers of barbed wire rising to the height of eight to ten feet), and protected by armed sentries, and also among other pieces of military equipment, were characterized by canvas tents comparable to those found in the ASI camps. At Dholavira, the impact of the camp as a powerful site wielding state power was strengthened by the presence of a helipad, which had been constructed when a General of the Indian army and his archaeologically inclined wife wanted to take a tour of the site in the late nineties. It had since become an important fixture of the camp, its utility limited to rare VIP visits, but its symbolic power adding substantially to the military–statist aura of the Dholavira ASI camp. My informants noted that during the Indo-Pak military build up during 2001-02, the Indian army had regularly used the helipad. The spatiality of the ASI camp was much more impressive than the more common statist architecture in the countryside and its semiotics was closely aligned with the daunting architectural manifestation of the military camp. This material similarity between the ASI and the military camps produced the unambiguous effect of a powerful commanding presence. The ASI camp's materiality, its architectural and spatial formation resonated with the ultra-authoritative aura of the state in the *(war)field*.

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[54] For example Baror was just 8 kilometers away from the Pakistan border whereas, Dholavira and Juni Kuran where were about between 50 - 60 kilometers away.

[55] The huge army build up during early 2002, during the near-war Indo-Pak confrontation, also largely contributed the ubiquity of the military camps in this area, some which continued to remain while I was doing my fieldwork.

The physical architecture of the camp was thus a material reflection of the terms of engagement of the state with its people. The practice of overtly exerting power over the local landscape and its people was obvious from the moment the camp was established. The architecture of the camp reflected the desire of the ASI to distance itself from the local community, and through its physical apparatus, it laid the ground-rules of contact. By just controlling the entry and exit of the local people into the camp, the ASI determined those on whom it bestowed its privileges and those it deemed fit to discard. While the physical boundaries of the camp were flimsy and could not protect the camp from any outside intervention or assault, its symbolic power was such that it effectively defined the local villagers' interaction with the ASI. The fence and the gate were banal symbols of power that the state exerted on the people, but they had a tremendous impact on the way in which the local people[56] interacted with the ASI. Having its genesis in the colonial military camp, the ASI camp was deeply rooted in the oppressive and dominating logic of such an organizational structure. The structural logic of the ASI camp mirrored the principles of the military camp, which had the desired effect of producing an ultra-official bureaucratic impression. The prevalence of this colonial organizational structure in the postcolony must have been because of the explicit ideological impact that the ASI wished to exert over the landscape and its people. Further, this structure revealed how deeply the ASI itself was still trapped within the structural logic of the colony. The ASI's intervention in the countryside was temporary, and therefore it was important for it to produce a powerful impact in a short time, and the model of the colonial military camp was the ideal choice, or probably the only choice imaginable to effect such an impact. This was the subtext underlying the ASI's choice to construct an authoritative spatial formation rather than to underplay its identity by utilizing the infrastructure of the local village. The camp was the architectural representation of the state's power at the fringes of the nation, and its genealogy was deeply embedded in the military expansion of the colonial empire. In the postcolonial space the camp represented the dominant authority of the state and its architectural spatiality not only characterized the nature of the state's ideological intervention in the landscape but also defined the norms of social interaction with the people that it intended to work with. The camp played the role of a spatial conduit of immense authority between the local people and their engagement with the

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[56] By local people I primarily mean the labor force and others who had direct interaction with the ASI and whose daily lives were impacted by the excavation's political economy.

archaeological project. However, the spatiality of the camp not only delineated how the outside world interacted with the ASI camp, it also had a powerful impact on how the inhabitants of the camp interacted among themselves. The architectural design of the camp also had another formidable function - that of disciplining its own residents - the ASI archaeologists and staff.

The camp and the semiotics of hierarchy

The social dynamics of the camp were vastly different from those of the head office. Unlike the office, which was framed within temporal terms as a *nine-to-five job*; the camp involved a twenty-four hour constant engagement with the ASI and its disciplinarian establishment. All the members of the excavation unit, from the director to the student labor, were aware that the excavation camp was a distinctive form of official engagement governed by a different set of norms and mores, albeit framed within the disciplinarian logic of the office. One of the important distinctions between the excavation site and the campsite was in the context of disciplinary mechanism. At the excavation site, the labor was the site for the enactment of the disciplinarian apparatus of archaeology and bureaucracy, whereas at the campsite it was the ASI employees who were at the receiving end. As I have tried to show, the spatial formation of the campsite was a product of the ideology of the bureaucratic state, which differentiated not only between the state and its subject, but also nurtured a hierarchal organization within its folds. In this section, I will show how this hierarchy was reflected in the spatial organization and reproduced in the architecture of the ASI camp.

The ASI camp, as a socio-spatial formation, was the site where official hierarchy was produced and disciplinarian regimes of control over employees were exercised. Above, I have illustrated the way in which the location and the architecture of the camp reflected the statist urge to differentiate itself from the local people. Now I will describe how the camp's design reinstated the hierarchy of the head office and augmented the bureaucratic system. The camp was a spatialized and performative product of the disciplinarian logic of the statist office, but its impact was far greater, as it was at once the official *and* the residential headquarters of the archaeological unit in the field. Its spatial formation and architecture had a mimetic relationship with the British military cantonment. Here hierarchy was clearly demarcated in its geography and reflected in its materiality. The social and the official relationships within the ASI were recreated and iterated through this spatialized hierarchy. While a different set of

social negotiations governed these official relationships in the field, the essential structure was similar. The camp recreated the hierarchy of the *head office* in a number of ways, made apparent in the daily practice of the ASI staff. Although these hierarchies and disciplinarian structures were constantly negotiated and subverted by all members of the staff, its impact on the daily life of the excavation members were ubiquitous. First, let us see how the physical structure of camp and its materiality reinforced the bureaucratic hierarchy. I would like to clarify that in this section I am not discussing the reinforcement and the impact of the disciplinarian practice of the ASI on labor. Here, I am specifically concerned with hierarchies that were produced in the architectural and spatial design of the camp, and demonstrate that it was iterative and constantly reproduced the disciplinarian oppression of a bureaucratic system.

Each ASI camp consisted of more than a dozen tents of varying sizes, erected on a ground plan, and its spatial formation reproduced the official hierarchy. In a very apparent manner, the sizes and the quality of the tents reflected the status of the members of the camp. The director and the officer occupied larger tents, in comparison to the tents occupied by the other members of the staff and the students - sometimes old and in tatters. Two to six occupants shared almost all tents, while the Director of the excavation, in contrast, had a tent to himself. The tents were placed in such a way that almost all members of the camp could see each other's tents, which had a rudimentary panoptical effect (see figs). This was created by segregating the tents that belonged to the students, the staff, the officers, and the director in different sections of the camp. In certain cases, as in Dholavira, the ASI officers lived in semi permanent *Boongas* – a traditional habitation unit, circular in shape, made of stone and mud with a thatched roof. The rest of the staff and students lived in tents, some of which were around 25 years old and were literally in tatters. In this architecture of hierarchy, the director's tent was special. Most noticeably, the size of the Director's tent was large, and in Baror and Dholavira, the director did not live in the tent but in the *mud house*. The semiotics of the mud house and the *Boongas* as luxurious living space is noteworthy, especially in the climatic context of the Harappan sites. The *Boongas* and the mud houses were both semi-permanent structures, which could withstand the extreme temperature in these arid parts of India, where temperatures would shoot to more than forty-five degrees centigrade in the day (in summers) and dip to around two degrees centigrade at night (in winters). Under these circumstances, the tents failed miserably as adequate shelters. In these extreme environments, the Boongas and the mud huts constituted elite habitation spaces. Within this spatial dynamics of the ASI camp,

the Director's tent had a far more exclusive position. Through its location, it served a panoptical purpose; it was situated in a special position within the precincts of the camp that allowed the occupant a clear view of most members of the camp. It was said of the Excavation Director at Dholavira, that everyday, early in the morning, at the start of the excavation (usually 7 am) he surveyed from his tent the members of the excavation team who were late in going to the excavation site. A number of informants told me that excavation directors from the days of Wheeler had adopted this surveillance practice as a way of disciplining the staff and the students residing in the archaeological camp.

The interior of these habitation spaces entrenched the hierarchy of the bureaucratic convention, reflected in two primary forms of dwelling spaces – the bathroom and the toilet. These are intimate spaces in any form of modern dwelling, and in the ASI camps, they were important sites for the performance of hierarchy. Generally, the temporary nature of the camps does not allow for luxurious zones to carry out intimate acts of body care, but in all the camps, a rudimentary form of such spaces did exist. Their place within the larger domain of an archaeological project was banal and minor, but I was informed a number of times by ASI officers, that their proper construction and regular maintenance was an important part of the daily practice of camp life. All the tents were divided clearly into two spaces, the larger was the living cum bed room and consisted of two iron cots, and a couple of tables for personal effects. The bathroom was an enclosed region situated behind each tent where the laborers working in the camp[57] would every morning and evening dutifully leave two buckets of hot water. At Dholavira, the presence of running tap water in this portion of the tent, along with cement flooring, was responsible for making it a *VIP camp*. The toilets in most camps were temporary dugouts, usually located outside the camp's boundary, enclosed by a tent, which was moved once the dugout filled up. Here, hierarchy was inscribed in the form of exclusive dugouts for the Directors and senior officers of the camp. At Dholavira, the toilets were permanent structures *(pucca laterine)* made of cement and brick, which had modern ceramic commodes, used by all members of the camp except the Directors, and senior officers who lived in the *boongas* or the mud house, which had an attached toilet and bathroom. In the *wild* environs of the ASI camps, the prevalence of permanent structures with attached toilets and bathrooms, or even the presence of a tap in a tent assumed great *prestige value*. They became

[57] As opposed to those working in the excavation, and there was clear distinction between both these kinds of work, even though the monetary payment for all kind of daily wage labor was same.

important elements in the effort of domesticating the field by populating it with material objects and the physical constitution of middle class urban India.

The Director's habitation, either in the form of a tent or a mud house, had a special place in the camp - it was the absolute center of bureaucratic power in the field. In addition to its strategic location, and the special building material that it was made of, the interior of this space contrasted with other tents in its extravagance. I had the privilege to enter these habitations occupied by the director, and noticed a marked difference in the interior of these spaces. They were much larger than the usual tents (12 x 12 feet or 10 x10 feet). They often contained sofas and large wooden beds as compared to the iron cots that the rest of the staff slept on. An obvious luxury item was the television set (with a VCD player). In a couple of camps, the TV was kept in a common tent where any member of the camp could go and watch it; however in Bhirrana[58] and Baror, it was not meant for public viewing and was usually in the tent of the Director. It was only brought out during special occasions, like the cricket matches between India and Pakistan that were being played during the days that I was doing my fieldwork (2004, 2005 series).

Within this disciplinary regime, depending on the disposition of the excavation director, various other disciplining symbols were used to re-inscribe the hierarchy among the ASI staff. At Juni Kuran, for example, I observed that in front of each tent a small wooden plank, fixed on an iron rod, had been fixed into the ground announcing the official position of the occupant, and not the name. Painted in white on a black background (standard bureaucratic iconography) the designated position of the tent occupant was mentioned both in English (Roman script) and Hindi (Devnagari script): driver [*vahan chalak*], chief-photographer [*pramukh chhaya-chitrakaar*], chief artist [*pramukh chitrakaar*], camp director [*utkhanan sanchalak*], cook [*bawarchi*], guest [*athiti*], and others. This form of official declarative nomenclature, was typically found throughout the bureaucratic offices in post-colonial India, where the nomenclatural emphasis was on the position of the person rather than to the name. In the campsite its usage was to underscore the official domain of the camp, and was an iterative mechanism to enforce discipline in the camp. The disciplinarian ideology of such a nomenclatural exercise at June Kuran was taken to an absurd level where even spaces like kitchen [*rasoi*], dining room [*bhojanalaya*], toilets [*shauchalaya*] were marked in such a

[58] There was smaller black and white television that usually was undergoing repairs.

fashion. The iterative function of these signs was to transform the domesticated spatiality of the camp into an official space, and to leave no area in the camp untouched by officialdom. Another manifestation of this disciplinarian ideology was the idea of uniform. At Juni Kuran, the Excavation Director insisted that each member of the ASI staff wear a blue *Khadi* jacket, to distinguish them from the laborers. This insistence on dress code was unusual even by ASI standard, as the Co-Director of Dholavira told me. However its prevalence appeared to be a vestige of the military disciplinarian subtext that lingered on in the archaeological project. The ideological impetus of the uniform dress was similarly iterative and a mechanism to entrench bureaucratic discipline on the members of the archaeological project.

The temporality of the camp was dictated by the disciplinary regime of the factory time - its materiality was symbolized by the hour bell that was situated in all the ASI camps. This bell was usually situated near the gate of the encampment and was under the charge of the gate sentry [*chowkidar*] who rang the bell at every hour, 24 hours a day, like a clock tower, with the number of strikes signifying the hour. The bell was usually made up of an iron bar, which was struck by the sentry with an iron hammer. However this rule was broken four times a day-announcing the start of the morning shift, the mid day break, the afternoon shift, and the end of the afternoon shift - at these times a flurry of strikes on the iron bar announced the importance of that particular hour. The power of this bell was astounding; like the factory siren, the bell was a means of controlling both the laborers and the AS1 staff who lived in the camp. It governed their daily life even after the end of the day's work when they were resting in the middle of the night. Not only did most ASI staff members have wristwatches, but also many laborers at the site owned one, including women. Therefore, the valence of the bell was not located in its utilitarian value but in its disciplinarian significance. The sound of the hourly bell was employed to control the temporal universe of the inmates of the camp. It was a constant reminder that even though the ASI excavation camp was located in the outskirts of the civilized nation, the disciplinarian mechanism of the bureaucratic office still controlled their life. While the ASI members defended the usage of the bell as a means to control the labor that worked at the camp, it was obvious that it also controlled the temporal universe of the very ASI staff who justified the usage of the bell. As an ASI staff member poignantly told me, the ubiquitous sound of the bell was so powerful that it even infiltrated into their most pleasant dreams, making them aware that they were part of an ASI excavation camp [*sali* (a common explicit), *sapne mein bhi hame ASI ki ghanti chorthi nahi hai*]. Thus, the campsite,

unlike the head office in the metropole, was a twentyfour hour engagement with the disciplinarian structure of the bureaucratic system leaving no room for non-official time or space.

The archaeological camp was not just a spatial formation that powerfully articulated the distinction between the state and the people it governed but it was also a disciplinary formation, which institutionalized bureaucratic difference and hierarchy through the spatial and even temporal organization of the camp life. The camp was not the office, but its daily life was controlled by the disciplinarian regime of the office – it was an ideological mimesis of the office. The state in the field did not have any model other than the bureaucratic office in the metropole to impose its social formation, to domesticate the wild landscape, and to control not just the laborers employed but also to regulate its own inmates in the wilderness of the field. This practice was the primary mode of engagement of the ASI as a post colonial bureaucratic system- it defined the work culture at the site, and eventually had a powerful impact on the ways in which knowledge was created at the excavation site. This overwhelming spatial hierarchy was the defining feature of the ASI's formation of the field, and, as we will see in the next chapter, it complemented the hierarchical relationships that structured the social life of the archaeological field.

Conclusion

In this chapter, I have shown how the ASI structures the space of the archaeological field and produces a statist space before the excavation process commences. The goal of this chapter was to critically investigate the non-epistemic domain of the archaeological field, which usually slips through the crevices in the literature on archaeological method and theory. These are pre-theoretical, taken for granted, elements of the archaeological field, but they appear to frame the epistemological intervention of the archaeological project. Their actual impact on the knowledge produced in the archaeological field has been ignored, and they have often been dismissed as too social and cultural to be discussed within the methodological process of the archaeological practice. I have tried to show that these non-epistemic practices in the archaeological field are central to the conceptualization of the archaeological project. In isolating these process through which the ASI archaeologists tame their landscape and transform it into a conducive social sphere, domesticate it into a version as close as possible to the offices they inhabit, I was interested in explicating how the archaeological project, from

the moment of its inception, is an ideological engagement with the spatiality of landscape. The ideology of spatial transformation was not an isolated product of postcolonial ASI intervention in the archaeological field; it resonated with the hierarchical social sphere that the ASI archaeological project generates in the field, as we shall see in the next chapter.

Chapter 4

The Epistemological Formation of the Excavation Site

Introduction

It had already been a few days at Dholavira. I was gradually coming to know the members of the ASI staff and the core workers at the site at a more personal level. My daily routine began with a quick breakfast following which I joined the staff and the students as they made their way to the excavation site. I usually accompanied the Asst. Archaeologists and sometimes the co-Director, as they went on their daily "inspection round." By the time we arrived at the excavation trenches, all the students and ex-students who were in charge of the quadrants were already at their respective locations, planning the day's work. The laborers were usually the first to come to the site. They started their work, typically cleaning the trench floor or clearing the dirt, preparing for the next "dig", under the supervision of the trench leader. Because Dholavira was a huge site, the inspection round took up almost the whole day as we moved from one mound to another from the citadel to the middle town, the lower town, the various reservoirs, and the burial grounds.

At the end of the day, as a ritual, I would return to the citadel along with the Asst. Archaeologists and sit atop the highest point on the mound – at the edge of the fortification wall. This custom, an Asst. Archaeologist who was a veteran at Dholavira, pointed out to me, was an "old one." It had originated in the early seasons of excavation when the citadel had yet to be discovered and excavated. Perched atop the fortification wall, sitting on ancient limestone blocks, carved more than three millennia ago, we could see almost the whole site, across the Rann with its surreal snow-white salt water marsh stretching into oblivion. We remained there, admiring the view as the sun set in the horizon, its glimmering rays transforming the paleness of the still water into a golden sheet. Soon the makeshift bell made out of an iron cylinder, signaling the end of the day's work, was struck by the *chowkidar* Bholla Bhai - one of the senior-most labors at the site - an old, weary man with a massive ash colored moustache wearing a turban, a bush shirt and a dhoti. Supporting his frail gait with a wooden shaft, he walked from the camp to the top most part of the citadel from where every one could see him and hear him. Soon from the trenches arose a small army of fatigued laborers - men and women, gradually emerging from the sunken squares in the earth, treading towards the camp, their clothes covered in dust and soil. The men bent over with the heavy weight of the tools of their trade on their rugged shoulders – spades, pickaxes, brushes,

trowels and knives. The women were carrying wicker baskets on their heads crammed with the pottery-shards and bone-fragments that had been excavated that day. Alongside walked exhausted site supervisors and students bearing backpacks with some of the more precious recovered artifacts. It looked like a pre-industrial agrarian ritual, reminiscent of farmers and pastoralists returning home after a hard day's work, "In Hindi this time [*waqt*] of the day is often called *godhuli,*" mused an Asst. Archaeologist, who had grown up in a small village in north India, "the name comes from the cloud of dust [*dhuli*] raised by cattle when they return home at dusk". During such nostalgia-evoking moments I was often reminded by my informants with an air of finality and definiteness "this is what ASI archaeology is." Sometimes it was one of the Asst. Archaeologists, at other times the draughtsman or the photographer and once even the co-Director had gestured, with an air of gentle pride: "the ability to dig such huge areas, teeming with hundreds of laborers working in precision. Producing *perfect knowledge* about the past." (the phrase prefect knowledge was uttered in English). It is this quest for the perfect knowledge about the past and its production through the process of archaeological excavation, as explicitly practiced by the ASI that will be the focus of this and the subsequent chapters in this dissertation. In this chapter, I will specifically show how a "wild landscape" located in the middle of rural India is transformed into a *materialized epistemic space* (Rose-Redwood 2006: 84) - ideologically laden abstract spatiality - suitable for the production of "perfect knowledge about past."

In chapters 3 I demonstrated that contrary to most theorists of archaeology, the field was not merely an epistemic location where knowledge was discovered and created but also a socio-cultural spatial zone, which encompassed deeply consequential, non-epistemic locations like the camp. However, the archaeological field and the act of the fieldwork carried deep epistemological significance since they were essential in the task of discovering, gathering and producing archaeological knowledge. In this and the subsequent chapters, I will focus on how the spatio-epistemological entity known as the archaeological field was conceptualized and constructed by the ASI, and articulated through the practice of fieldwork. The preparation and the reconfiguration of an anonymous space (discovered during exploratory archaeological work a number of years before) into an iconic epistemic location 'the excavation site' is the subject of this chapter. I particularly want to explicate the *ideological* process responsible for this transformation of an unexcavated archaeological site into an epistemic site suitable for the production of objective knowledge.

Excavation Site

Central to the idea of archaeological fieldwork as the ASI imagined and articulated it, was the excavation site. This was a rigidly defined knowledge production location where scientific archaeology was performed and enacted. This was the location at which the act of unearthing the earth to recover the material cultural deposits buried underneath was carried out. Ever since the days of the J.A Worsaae in the middle of nineteenth century, when scientific archaeology was first undertaken in small attempts (Triger 1981: 80-81), the excavation site had been conceived as an explicit spatiality where it was possible to produce scientific knowledge only through the physical removal of the surface. It was at this location that archaeological artifacts were first discovered, recognized, produced, and categorized, producing the first instances of archaeological knowledge. Although exploratory fieldwork also contributed to producing knowledge, it was only through the process of excavation that artifacts could be definitely fixed in a precise spatiotemporal matrix. This ability to fixate material artifacts in an exact time-space continuum made archaeological excavation the most preferred form of archaeological intervention for establishing credible evidence about the past. Knowledge produced at an excavation site was considered unfaultable and had the valance of genuine knowledge, giving the excavation site a privileged epistemological place within the discursive framework of archaeology. Excavation-based evidence was factual, precise, and irrefutable. Post-processual archaeological theory has aggressively argued that the significance of knowledge produced at the excavation site is primarily interpretative; however the excavation has not been de-stabilized from its central role as the producer of archaeological knowledge.

The process through which a spatialized location on a landscape, which has been identified to have potential for archaeological intervention and designated as an archaeological site, was transformed into an excavation site, involved a highly codified mechanism of inscribing on the landscape an *ideo-epistemological* formation. I argue in this chapter that that the preparation of a landscape for archaeological excavation was an ideologically driven practice through which an unknown landscape was rationalized and domesticated by the disciplinarian configuration of science. Within the discursive domain of ASI's archaeological practice, this was carried out by imposing on the landscape the visual configuration of the Cartesian grid, which had been introduced into Indian archaeology through the intervention of Sir Mortimer

Wheeler.

The Wheelerian grid was a practice through which an anonymous landscape was rationalized and brought within the encompassing grasp of Cartesian perspectivalism, a practice that had both aesthetic and disciplinarian affect. The Wheelerian grid signified the *scientific-ness* of archaeological practice as carried out by the ASI. Similar to the way in which the ASI cleared up civilian agricultural areas to create a statist camp, the process of creating an epistemic space out of a barren landscape involved a profound ideological program. It was also an engagement with the land and landscape, but driven by the need for a systematic and scientific acquisition of data. This data-oriented process of the ASI project re-organized the landscape into an epistemic spatiality, which was both a product of Latour's "inscriptive practices" (Latour 1987), Lefebvre's "representational space" (Lefebvre 1991) and Foucalidian "disciplinary spatiality" (Foucault 1977). Here the wilderness of the physical landscape, through the materialized epistemic practices epitomized by Wheeler's Method, was re-structured into a spatiality of symbolic value. Here the division between materiality, epistemology, and ontology merged to produce a disciplinarian spatiality, which not only functioned to extract scientific knowledge but also disciplined the ontological universe of those who inhabited the disciplined space.

The Wheelerian Grid

The visual impact of an ASI archaeological site under excavation was formidable. These Harappan sites were nestled in the dry and flat riverbeds hidden behind the dusty countryside of western India. Some lay in the rugged deserted landscape of Kutch surrounded by low-lying salt-water marshes, where large colonies of *babool* (*acacia tortilis*) scrub forest threatened to overrun any human occupation (Dholavira).[59] Others sites were tucked in the sprawling agrarian countryside between rambling farmsteads and expansive sugarcane plantations (Bhirrana, Baror), or constricted by congested villages, jostling for space, encroaching and intruding into the territory of the *protected monument* (Hansi). At each of these locations, when I walked from the camp into the excavation site I encountered the characteristic mound

[59] The species *acacia tortilis* found in Kutch was native to Israel. It was introduced in India as a way to counter desertification in the 1950's-60s. Its plantations were set up in various parts of deserted area of western India in Rajasthan and Gujarat for "sand dune stabilization" (Sinha et al 1997: 116). The local population considered it to be very invasive species and often local informants told me that "it has ruined Kutch" [*Kutch ka satyanash kar diya*], and had been nick named "crazy balool" [*pagal balool*] because of its unrestricted growth and proliferation.

shaped by the archaeological intervention typical to this part of the world - the earth divided into a neat arrangement of squares laid out geometrically. The long slender pathways crisscrossing each other at right angles, clutching the earth under their powerful gridlock, inspired awe. Between these pathways were carefully and painstakingly dug gaping square holes of varying depths, proliferating the expanse of the mound. The landscape, devoid of any foliage, was inscribed with the famous Cartesian network known amongst archaeologists in India as the "Wheelerian grid". Although my association with Indian archaeology had been ten years, I first saw huge archaeological sites divided into Wheelerian grids only during my fieldwork for this ethnographic research. My archaeological training in India had been at the Deccan College, which was critical of the large-scale horizontal excavation as practiced by the ASI. Methodologically, archaeologists at the Deccan College loathed the Wheelerian grid and accused the ASI archaeologists of being "slavish to Wheeler's method of archaeological excavation"- a remark I vividly remember being uttered by a professor of mine during a graduate course on archaeological methods. Seeing the earth carved into squares, divided by "balks," never failed to overwhelm me because it represented a powerful way of transforming earth into a systematic landscape

Since the excavation of Mohenjodaro in the 1920s, almost all mature Harappan sites have seen large-scale horizontal excavation. This extensive unearthing of sites represented the epitome of *classical* archaeological intervention of the ASI. The grid network was the embodiment of both the scientific innovation that Sir Mortimer Wheeler introduced to stabilize the wavering ASI between 1944-48, and also of the disciplinarian regime that he established during his four-year tenure. Archaeological excavation before Wheeler was regarded as "unscientific excavation" and "arbitrary excavation" (Rajan 2002: 84), and it was only with Wheeler that excavation in a gridded network was initiated. Methodologically, the grid epitomized the scientific practice through which archaeological excavation was conducted by the ASI and symbolically it represented the disciplined nature of the archaeological act conducted by the colonial statist regime. This methodology of transforming ancient sites into locations for the production of scientific knowledge gained popular currency as the 'Wheeler method,' and became standard practice in the trenches of India, especially in the ASI. Wheeler's most important contribution to this technique was dividing the archaeological site in grids and inscribing it with Cartesian co-ordinates in the form of balks. This divided the earlier chaotic location of knowledge production into a scientific laboratory held together by balks, whereby

the generated information could be confined, controlled, and codified. In this archaeological laboratory, facts about pasts could be scientifically documented and accurately retrieved by keeping a detailed three-dimensional record of the finds. The carved out laboratory space in the earth provided stratigraphical indices whereby this retrieved evidence about the past could be further systematized according to Cartesian coordinates.

I began my fieldwork after having done extensive archival work about the nature of Wheeler's intervention in the ASI. I was interested in investigating how the disciplinarian discourse that Wheeler was instrumental in inserting into the ASI was played out in daily practice. I was well aware of the impact of Wheeler's intervention, and the aura of Wheeler that permeated in every stratum of the ASI bureaucracy -- from the students at the Institute where Wheeler's persona was regularly recreated in classroom settings, to the numerous ex-Director Generals of the ASI, who would often invoke Wheeler's contribution during public functions when describing (for instance) the role of the ASI in discovering the rich archaeological heritage of India. Wheeler and his contribution foregrounded every aspect of the ethnographic work that I was involved in. However it was only during fieldwork that I realized the enormous impact that Wheeler's intervention had had on the archaeological practices of the ASI. The influence was not restricted to the way that the ASI conducted archaeological excavation, but Wheeler's disciplinarian ideology also extended to the bureaucratic relationships between various members of the ASI in the context of knowledge production and knowledge representation. It was disappointing to observe that postcolonial archaeological practice articulated by the ASI in the early twenty-first century was a replica of the disciplinarian regime that Wheeler was instrumental in launching in India in the 1940s. ASI's practice was nothing but an ideological and methodological mimesis of a project established by a colonial scholar-bureaucrat. The ASI employed almost the same strategies and methodological procedures to excavate, discover, and produce archaeological knowledge as had been employed sixty years ago. It would not be an exaggeration to say that the ethnographic descriptions of archaeological practice that offered in these pages could be those of the days when Wheeler himself had worked in the dusty and rugged terrain of north-western South Asia at the sites of Taxila and Harrapa. Before I begin to describe and theoretically locate the ideological subtext underlying the articulation and performance of ASI's archaeological practice, it would not be inappropriate to discuss Wheeler's contribution to Indian archaeology, and especially to the ASI during his four year tenure as the Director General of the ASI.

Sir Mortimer Wheeler and Disciplining Colonial ASI

For Mortimer Wheeler, the news of his appointment as the Director General of Archaeology in India was a 'complete bombshell,' an apt metaphor to be used by a Brigadier in the 42nd Light Anti-Aircraft Regiment of the British army. Recalling the day in early August 1943, Wheeler writes in his memoirs:

> In the sunset, at the end of the day's planning operations of the forthcoming British and American invasion of Italy had drawn to its just close, when the Corps Commander, General Sir Brian Horrocks, dashed across towards my doorway with a signal in his hand and the remark, 'I say, have you seen this - they want you as - [reading] - 'Director General of Archaeology in India!' – Why, you must be rather a king-pin at this sort of thing! You know, I thought you were a regular soldier! (Wheeler, 1976: 9)

Wheeler was summoned to head 'the largest and the most complex archaeological machine in the world' (Wheeler, 1956: 179) which had 'notoriously at that time fallen into complete disrepute' (Wheeler, 1976: 10). This disrepute stemmed from the disorganized state of the ASI, perpetuated by the inability of weak successors (after the retirement of Sir John Marshall in 1926) to keep the disintegrating colonial agency cohesive. This led to the appointment of a one-man committee of Sir Leonard Woolley to investigate the nature of the decay. Sir Leonard Woolley, in his critical report of 1939, notes damningly:

> The department has been starved financially and more money must be spent if its work of exploration is to be put upon a proper basis; that is a fact, which cannot be gainsaid. But a mere increase of the grant accompanied by other changes would actually do more harm than good, for the truth is that the Department is altogether lacking in men trained for the work which they have to do (Woolley, 1993 [1939]: 20).

When this report was made public, it was not without controversy; it was very swiftly suppressed and never published (Possehl 1993: 1; Wheeler 1956:184; Chakrabarti 1988: 174).[60] Officers of the ASI and members of the Indian archaeological community protested

[60] K.N Dikshit who was the D.G. of the ASI during the period when Woolley wrote the report sought the comments of numerous academics and senior archaeologists to comment on the report and critique it. Amongst the most prominent archaeologists camping in India at that time was Aurel Stien who responded to Dikshit's call and noted that he and his department should be anxious about the negative impact of the report and encouraged that the report be published, he wrote, in handwritten letter on 4th April, 1940: " In conclusion I wish to assure you that I fully appreciate the apprehensions expressed in

against the critique that Woolley leveled against the ASI and the process through which Woolley had come to his conclusions.[61] Most critics of Woolley's report were downright offended by his remarks and attacked Woolley for his superficial understanding of the Indian archaeological community:

> Sir Leonard has done a great injustice to the archaeologist in Indian (sic) and has entirely ignored the great advances made in our knowledge of Indian archaeology by the results of the Archaeological Survey of India in stating that 'not even the skeleton of the cultural history of India existed' and that the sequence of cultural history was quite unknown" (AACD, File No. 1195/1940).

However the recommendations of the report were resurrected in a few years, especially in the context of the 'outside intervention', that Woolley had argued was necessary to transform the ailing state of the colonial ASI:

> In the matter of excavation, I have, on most sites which I have visited, found that the methods employed were bad, trained observation conspicuous by its absence, and the results in consequence, incomplete and untrustworthy...If the present efforts of the Department can be so characterized, it is manifest that the staff, before it can train others, must itself be trained; I therefore recommend the employment of a temporary Adviser on Archaeology who could deal with all the points at issue (Woolley, 1993 [1939]: 21).

It was to such a call that Wheeler heeded when he came to the Indian sub-continent for a four-year mission in February 1944 (Boast, 2002: 165; Paddayya, 1995: 134).

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the latter part of your letter as to the effect which Sir L. Woolley's Report might have upon the future of the Archaeological Survey. But I feel encouraged to hope that the opportune publication of that exhaustive and carefully prepared review of its work presented in 'Revealing India's Past' will remove any risk of serious set back in the attitude of Government towards the Survey" (AACD File No. 1195/1940)

[61] For example Woolley's comment on the state of excavation received the following retort "As regards Sir Leonard's observation on excavation, I must say that he was unable to watch any excavation actually in progress under a trained officer, such as Mr. Srivastava's work at Agroha. I fail to see how he could, therefore generalize about bad methods, want of trained observation and untrustworthy results' from what he might have noticed in a small scale trail pit being carried out by a departmental Draftsman at Sar Dheri, which he praised so far as the recording of antiquities and stratification was concerned" (AACD, File No. 1195/1940).

Wheeler was aware that his tenure was short and that he had a monumental task of transforming the ASI ahead of him, so he took up the task of disciplining the ASI right at the outset of his tenure. Recalling one of his first incidents of disciplining his staff, Wheeler in his professional autobiographical account "Still Digging," candidly notes:

> Had Jemdar Bagh Singh known the Revelation of St. John he might aptly have recalled the prophetic words: 'The Devil is come down amongst you having great wrath, because he knoweth that he hath but a short time.' The devil had in fact a four years' contract from the Viceroy in his pocket; though, as event shaped themselves, only three of those years were to be effective, the fourth being submerged in the turbulence and bloodshed of Partition (Wheeler, 1956: 186)

Working under this time constraint amidst the political turmoil surrounding the making of two new postcolonial nations, Wheeler restructured the ASI, because 'in that theoretical four years, nearly everything had to be done'; it was not 'merely a matter of reshaping, refinancing, revitalizing' but 'the dead wood of obsolete and erroneous ideas [that] had to be uprooted' (Wheeler, 1956: 186). There was a need 'to stir the activities of the Indian Archaeological Survey from its unworthy condition of lethargy and archaism to a new and modernized phase of archaeological research and methodology' (Wheeler, 1976: 32). Wheeler's tenure paved the way for the firm establishment of postcolonial ASI. Wheeler was responsible for transforming the institution from an administrative practice confined to maintaining and conserving monuments, running a few museums, collecting epigraphs, and conducting arbitrary excavations followed by intermittent publications, into a systematic academic exercise. This was done through the creation of a national museum and the founding of a journal, *Ancient India* (now discontinued). During his tenure, Indian archaeological practice was rejuvenated with the introduction of new strategies and problem-oriented fieldwork (Paddayya, 1995).

Scholars of Indian archaeology have often remarked on the importance of Wheeler's intervention (Clark 1979; Chakrabarti 1988; Paddayya 1995). Alluding simultaneously to the near impossible task that Wheeler accomplished and to the lethargic state of ASI bureaucracy during the last years of British rule in India, Paddayya notes that Wheeler's "term of office lasting four years (1944-48) was marked by a series of development which would normally take forty years" (Paddayya 1995:134). Chakrabarti remarks that Wheeler's "sense of archaeological planning and the excavation took Indian archaeology to a new level of

scientific awareness" (Chakrabarti 1988: 188). On the other hand, he also argues that contrary to popular perception, it would be incorrect assessment to "imagine" that "Mortimer Wheeler gave a "kiss of life" to Indian archaeology", because "Indian archaeology was not in its death-throes when he arrived in 1944 to stay on as the Director General" (Chakrabarti 1988: 188).

However, the aura of Wheeler in the ASI had been built not by these historical appraisals but mainly through oral tradition, ASI folklore that had been passed on by numerous senior archaeologists who had attended his famous training school in field archaeology in Taxila in 1944 and had gone on to occupy senior positions in postcolonial ASI - A. Ghosh, B.K. Thapar, B.B. Lal (retiring as Director General of ASI), S.R. Rao, and K.R. Srinivasan. I was told by several archaeologists that I met on the sites that they considered themselves to be direct descendants of Wheeler's intellectual heritage, which had been passed to them through his students. At Baror an ASA who had joined the ASI more than twenty years ago categorically stated, "we learnt archaeology the hard way, working day and night, excavating at sites like Kalibangan under Dr. B.B. Lal, who learnt the same way under Wheeler. And that is what we are teaching our students here. We are continuing the age old tradition of *guru-shishya parampara*."[62] Archaeologists and the ASI technical staff were not ambivalent about the importance of Wheeler. A senior photographer at Baror, who had been trained in the "art of archaeological photography" by his predecessor, who in turn had learnt to take "archaeological photographs" during "Sir Wheeler's time" reverently told me, " what we do in the ASI is a *ditto copy* of what Sir Wheeler started. He was the finest archaeologist India ever saw, and we merely follow his footstep. He was our guru" In the ASI, officers, staff and students, all consider his tenure as one of the most important periods in the history of twentieth century ASI. "Sir Wheeler virtually transformed the face of ASI and the way archaeological excavation was conducted. He made it scientific and modern" explained the senior photographer. When Wheeler came to India, the presiding figurehead of the ASI was Sir John Marshall, who had headed the ASI for a staggering 26 years from 1902. Wheeler was aware of the role that Marshall had played in the making of the ASI before he came and the aura of Marshall that pervaded the ASI when he, Wheeler was appointed: "Certain it is that, when I reached India in 1944, Marshall was still a remote king-god of whom his worshippers had no intelligent comprehension, and sought none' (Wheeler 1956:182). It was ironic to

[62] "Guru- shishya parampara" or the teacher-disciple tradition epitomizes the oral transference of knowledge in ancient India.

discover during my fieldwork, nearly sixty years after Wheeler had made his presence felt in the ASI, that I could repeat exactly the same words about Wheeler's demi-god like status in the ASI. And as I came to gradually realize while talking to my informants, this process of deifying archaeologist-scholars was not restricted to the likes of Wheeler, whose contribution (despite my criticism), was seminal and far-reaching, but even extended to Wheeler's students (such as the prolific B.B. Lal) and even to the Director of the Dholavira excavation, Dr. R.S. Bhist. The foremost reason for this creation of aura around Wheeler and other archaeologist scholars was their impact on students during the field school, which since the first started by Wheeler in 1944, had become a central part of the yearly ritual carried out by the ASI under the aegis of the Institute of Archaeology.

Often referred to as the "Taxila School of Archaeology" in the ASI, it was one of the most important field training schools in the history of South Asian archaeology and probably the first such in the world (Paddayya 1995:134). The first of these had been a crucial intellectual event orchestrated by Wheeler during his tenure and had been responsible for transforming the community of Indian archaeologists into scientists. It focused on training young students in 'the neglected arts of India's archaeological technology' (Wheeler, 1976: 32). This 'tiny academic episode' (Wheeler, 1976: 32) was the first organized school of field archaeology in South Asia and it played an influential role in the making of post-colonial Indian archaeology. Students from this training camp emerged to head various archaeological departments throughout the country. Some among them went on to head the ASI over the following decades (Chakrabarti, 1988: 176). This short event during Wheeler's residence was responsible for creating the aura of Wheeler as not only a competent administrator of a deteriorating colonial institution but also as a great teacher of archaeological methods. At Taxila and the other sites where he worked, such as Arikamedu, Brahmagiri, and Harappa, Wheeler inscribed on Indian archaeology, ideas of scientific excavation, the importance of stratigraphy, and other archaeological methods that became the basis for the production of knowledge for the ASI.

During the period between the two World Wars, Wheeler developed his techniques and ideas which were to be utilized to their fullest in the Indian context and which had a profound impact on the postcolonial articulation of Indian archaeology as a discipline. Influenced by the work of General Pitt Rivers (Lucas, 2001: 36), who at the end of the nineteenth century had

advocated for excavating ancient sites in a scientific manner, Wheeler transformed the archaeological endeavor into a militaristic exercise. He was driven by the need to turn the archaeological process into a professional practice that would produce scientific knowledge with rigor and precision. In doing so, Wheeler borrowed heavily from his military experience, aspiring to be both comprehensive and scientific in disciplining archeological practice and process. In order to create a chain of command that would produce knowledge, Wheeler argued for a "basic factor of labor-control or in the quaint terminology of the army, 'Man-management,'" which was 'very much the same thing' (Wheeler, 1954: 173). He further explained:

> In one vital respect at least there is an analogy between archaeological and military field-work that is recurrent and illuminating. The analogy rests - strangely enough as between the dead and the deadly - in the under-lying *humanity* of both the disciplines. The soldier, for his part, is fighting not against a block of colored squares on a war-map; he is fighting against a fellow being, with different but discoverable idiosyncrasies, which must be understood and allowed for in every reaction and manoeuvre. Equally...the archaeological excavator is not digging up *things,* he is digging up *people* (Wheeler, 1954: 16-17)

This analogy with the militaristic exercise is a recurring theme in Wheeler's thoughts, even more transparently seen in Wheeler's articulation of archaeological method and theory. Wheeler utilized the military trope for transforming an incipient disciplinary discourse, still viewed as a pseudo-scientific antiquarian's delight, into an empirical and scientific laboratory, capable of providing knowledge about mankind. Wheeler collapsed the ideas of scientific thought and military strategy into a single discourse that emerges as a constant subtext to all his archaeological approaches. As he categorically points out:

> It is no accident that leaders in their interpretation [of ancient fortification] have so often been soldiers; General Roy, for example, in the eighteenth century; General Pitt Rivers, Napoleon III's colleagues, and the distinguished officers, who manned the German *Limes* Commission in the nineteenth. Our hill-forts, as Leland long ago remarked, are the works of 'men of warre'; and their study demands the virile spark of the mind militant (Wheeler, 1954: 18)

For Wheeler, the archaeological project suffered from lethargy: a malaise that could only be cured by making it a more professional mechanism of knowledge production, akin to the scientific enterprise. This is evident in his concern with 'methodical digging for systematic

information, not with the upturning of earth in a hunt for the bones of saints and giants or the armoury of heroes, or just plainly for treasure' (Wheeler, 1954: 20). The genealogy of these concepts can be traced to Pitt Rivers (Wheeler, 1954: 13). Pitt Rivers had been the first to develop strategies for comprehensive excavations, which involved digging uniform and symmetrical trenches, divided by balks for maintaining stratigraphy and recording these finds three dimensionally according to their stratigraphical configuration (Trigger, 1989: 199, Lucas, 2001: 39).

Wheeler began shaping and building up on these ideas soon after the First World War, when he commenced work on the Roman and Iron Age sites in Essex and Wales (Lucas, 2001: 37). He was driven by the need to gather more data because the 'knowledge of human achievement outside the historical field was dependent upon fresh and methodological discovery, and that fresh discovery in great measure meant fresh digging' (Wheeler, 1956: 66). At the excavation of the prehistoric fortification of Maiden Castle in Dorset between 1934-37, Wheeler, for the first time, utilized the technique of area excavation in regularized trenches with baulks, along with the practice of meticulous mapping and recording of all significant features (Lucas, 2001: 39). He used stratigraphy, a concept introduced into the archaeological domain from geology, extensively in his excavations (Harris, 1989; Wheeler, 1947c: 143). This was a technique introduced by Pitt-Rivers as an important means of retrieving accurate and comprehensive scientific knowledge from an excavation, in order to establish internal chronology and the relative sequence of ancient cultures, a central pursuit in archaeology at the time (Lucas, 2001: 34; Trigger, 1989: 199; Wheeler, 1958: 55). Wheeler was an able inheritor of Pitt-Rivers' principles, not only because of the shared military background, but also because of their insistence on the discipline, rigor, and professionalism that both infused into archaeology. These principles were reflected in Wheeler's insistence on an accurate recording of the archaeological sequences, the finds, and the structures in accordance with their stratigraphical indices, which turned the previously chaotic knowledge production process into a meticulous exercise.

Wheeler evoked the use of the military metaphor; the archaeologist as a professional solider apprehends the archaeological site as a war zone, in which his superiority and domination are to be exhibited. In the first among a series of Staff Memoranda with Technical Sections that Wheeler wrote as the Director-General of the ASI, he described the principles of his method in the following way: 'the excavation of a site, like the ordering of the battle, must be thought and co-ordinated by a single present and directing mind. Otherwise chaos, waste, inefficiency

is inevitable' (AACD, File No. 33/24/44; 1944). The genesis of the Technical Sections lay in the Staff Memoranda, meant for internal circulation, that Wheeler dispatched throughout the early part of his Directorship between 1944 and 1945. Seven in number, these were his first attempts at disciplining the staff, the officers, and the workers of the ASI. It is through these succinctly titled Memos - 'Conservations', ' Research', 'Museums', 'Directives for Young Officers' and others, that he articulated his objective of putting to order a colonial organization in disarray by bringing together the virtues of science and discipline: 'Once more you are a scientist, one with the initiative to acquire and enlarge knowledge. You are no longer a school-boy waiting to be taught. You are an *officer*, and the weight of your command will be proportionate to the effective weight of your knowledge and experience. Learn!' (AACD, File No. 33/24/44). For Wheeler, science was like a war:

> [war]... is a whole-time preoccupation. It has nothing to do with office hours. There is no such thing as 'science from 10.30am to 5.30 pm'. Those are hours between which the administrating scientist has least time for his science. The real work begins when his routine work ceases. And archaeology is a branch of science (AACD, File No. 33/24/44; 1944).

Stratigraphy, like other excavation strategies characteristic of the 'Wheeler Method', performed a dual role. Firstly, it was a scientific tool to negotiate the vast mass of archaeological knowledge that needed organization and codification and to bring it under the control of chronology established by known Graeco-Roman records. Secondly, it was a way to professionalize Indian archaeological practice, by introducing a new level of scientific awareness that could bring system into the chaos (Chakrabarti, 1988: 88):

> To dig a site merely because it 'looks good' or because it might produce useful information would be comparable to carrying out a surgical operation at random on a patient in the hope of finding somewhere the cause of an undiagnosed disease. It was thus that the primitive surgeon used to cut a hole in a man's skull in the hope of letting out the headache. It is thus that ancient sites - megalithic tombs for example - have been constantly opened up in the hope of letting out their secrets. Not thus is the orderly way of science (Wheeler, 1949: 4)

Although Wheeler pursued the project of making archaeology scientific, he accepts its inherent limitations: 'as scientists, our life is founded on selection and decision. We like to think that selection and decision are objective and impersonal. What fools we are!' (Wheeler, 1950: 122). His schemes were attempts to make the excavation a rigorous process whereby the

knowledge that could be potentially gained from the site was not lost forever through this permanently destructive procedure of excavation. He believed that 'archaeology is primarily a fact-finding discipline' and that an archaeologist is:

> primarily a fact-finder, but his facts are the material records of human achievement; he is also, by that token, a humanist, and his secondary task is that of revivifying or humanizing his materials with a controlled imagination that inevitably partakes of the qualities of art and philosophy (Wheeler, 1954: 228-229)

It is this oscillation between the archaeologist as the scientific technician who gathered unsoiled data and the archaeologist as the humanist interpreter that informs Wheeler's archaeology. His main objective was to grasp the idea of Man, 'a subject which, being Men ourselves, we can never fully objectify. Our science is of all sciences the most subjective and selective' (Wheeler, 1950: 122). It is this epistemic and philosophical sway that marks Wheeler's work. On the one hand, his work concentrates on disciplining the chaotic practice of excavation, and on the other, it inscribes an idea of the human past on the site and its people (Wheeler, 1954: 80). It is in colonial India that both these projects reached their logical conclusion. It was in this location that Wheeler controlled and patronizingly trained the natives, while simultaneously continuing the Indological project of inscribing on the colonial masses, a past unknown to them (Wheeler, 1946c; 1947b; 1947-1948b; 1950; 1959; 1962a; 162b; 1968; 1966b).

The intervention of Sir Mortimer Wheeler has had a longstanding impact on the way knowledge about India's past is produced, especially in contemporary post-colonial India (see Paddayya, 1995; Clark, 1979; Chakrabarti, 1989). He transformed a decaying institution of colonial power into a professional organization, competent in pursuing the colonial project of codifying, classifying and endowing the colony with a past. In Wheeler's intervention, the colonial, the military, and the scientific projects were collapsed to form a disciplinarian discourse that resulted in the emergence and domination of the past as a cultural category. His methodological strategy of applying military and scientific procedures to produce an objective reality of the past, with a clearly defined chronology emphasized the ASI's status as a superior imperial body.. Wheeler's intervention was a transformative interlude in the trajectory of a colonial body whose roots lay in the knowledge production project that the British started, soon after they established a firm political grip over this vast colony in the first half of the nineteenth century (Lahiri 2000). Taking up the helm of a colonial agency like the ASI, which

had been established in order to produce knowledge about a newly conquered colony for enabling adequate governance and subjugation, Wheeler was successful in transforming it into an efficient instrument of the post-colonial state that was capable of rigorously producing scientific facts about India's past. In the following section, I will demonstrate how Wheeler's intervention led to the re-organizing of the archaeological landscape as an excavation site and how Wheeler's militaristic and disciplinarian framework was instrumental in making the excavation site a formidable artifact of scientific archaeology.

Large Scale Excavation

The Wheelerian grid, according to ASI archaeologists, was the most appropriate way to dig an archaeological site, and especially an ASI archaeological site, because of the "large-scale" nature ASI excavations. My informants often used the term "large scale," to describe ASI interventions. For them "large scale" aptly reflected the industrial and technocratic nature of the archaeological work that the ASI pursued. The term was uttered with an air of pride and it seemed to also subtly allude to the ASI's nationalistic project with its wide-ranging political and historical ramifications. "Large-scale" meant big excavation, both in terms of the financial and governmental resources that the ASI had at its disposal and the physical magnitude of the excavation. The term was often contrasted with "small-scale" university department excavations, and the excavations carried out by the various regional (although, also statist) archaeological organizations. The semiotics of the term large-scale was distantly rooted in the ideology underlying the post-independence Nehruvian paradigm of Soviet style industrial development adopted by India, embodied in the form of big dams and large state controlled industrial factories, mills, and manufacturing units. Within that same semiotic universe, the term "small-scale", referred to village based traditional industries advocated as the sustainable economic paradigm for India by Gandhi. In the parlance of ASI archaeologists, both terms shared these semiotic roots and political genealogy. "We are the ASI. We are a central governmental organization and we specialize in excavating large sites. The University departments and the state departments just don't have enough money or the resources to conduct excavation in the way that ASI does" [*hum ASI hai. Hum central government organization hai aur hum bade excavation karthe hai. Yehi hamari specialty hai*]. Informants from the senior-most ASI officers to the junior most attendants at the excavation site commonly described the ASI's role in these terms. Large-scale excavations symbolized the definitive postcolonial statist intervention for the ASI. This was only the "proper way" for the

most important statist archaeological organization in India to conduct its business. An often repeated refrain was" "if we don't do it who will? This is our duty." [*agar hum nahi karenge toh kon karega. Yeh hamaari* duty *hai*]. Large-scale excavation was framed by my informants as the essential defining practice of the ASI as a statist archaeological organization. This sense of duty emerged from the ASI's self-perception that it was the only archaeological institution in India capable of undertaking large-scale excavation. Informants, one after another, justified the necessity of excavating large sites by citing the statist resources that were at the ASI's disposal and its "organizational capabilities and scientific expertise." They also laid claim to the historical legacy and experience of excavating large sites. "Its in our blood," an Asst. Archaeologist compellingly told me as we were sitting on a charpoy under the cool shade of a large banyan tree in the scorching heat, observing the laborers doing "routine work."

This was not an unusual way to justify the large-scale archaeological intervention of the ASI. The legacy of the ASI from Marshall to Wheeler to Lal, and finally to Bisht, was well entrenched in the rank and file of the ASI excavation machinery. These archaeologists were associated with the excavation of enormous sites like Taxila, Mohenjo Daro, Harappa, Ahichchhatra, Arikemedu, Kalibangan, Dholavira, which have become iconic representations of the institutional biography of the ASI. Ever since the days of Sir John Marshall (early 1900s), who had received his training at the large archaeological sites of the Classical world in the Mediterranean (Chakrabarti 1988: 122; Lahiri 2005: 46), ASI excavations were large and involved massive removal of the earth in order to uncover the ruins of the "great buried cities of antiquity" (Chakrabarti 1988: 128). Here

> [F]or the first time in India, well preserved remains of houses, shops and street, dating as far back as the Mauryan epoch, were laid bare, and numerous minor antiquities removed, which helps us materially to visualize the everyday life of the towns-people in those early days. These discoveries gave a promise of a still richer spoil awaiting the spade at the more important centers of ancient civilization (Chakrabarti 1988: 129)

For the ASI, ancient civilization was materially embodied in the form of monumental sites that explicitly represented the *culture* of ancient India. Under Marshall, the ASI, in accordance with trends in early 20th century archaeology, was obsessed with the idea of ancient civilization. And when the sites of Mohenjo Daro and Harappa were excavated in the 1920s, the archaeological intervention had to be large-scale in nature. This was but "natural" for Marshall, who had already been excavating Taxila for more than a decade when the Indus

civilization was discovered (Chakrabarti 1988; Lahiri 2005). The excavation at Mohenjo Daro under his leadership, followed by Madao Sarup Vats and Ernst Mackays's excavations at Harappa were all large-scale projects (Vats 1940; Mackay 1938), involving multi-year engagement, uncovering huge tracts of earth. When Wheeler entered the scene, his archaeological intervention was no different - Taxila. Harappa, Brahmagiri, and Arikamedu were all large-scale excavations. And this tradition was followed by the Wheeler's predecessors (and students) in the postcolonial ASI, exemplified in the famed large-scale excavations conducted by B.K. Thapar (Maski, Mathura), B.B. Lal (Kalibangan, Sringaverpur, Purana Qila, Delhi), and many others.

By the time I was doing my ethnography, large-scale excavation had become the norm, especially for excavations conducted by Excavation Branches. This does not mean that small-scale excavation was rarely undertaken. These were usually carried out as exploratory excavations, in order to determine the potential of a site before the large–scale excavation. These small-scale excavations were termed as "scientific clearance" in the bureaucratic parlance of the ASI. These basically consisted of trial trenches and surgical trenches to study the stratigraphical chronology of a site or a mound and were usually carried out a season or two before a scheduled large-scale excavation. Large scale excavations were the hallmark of the ASI's practice of archaeology and contrasted sharply with excavations conducted by Indian university departments, which were in most cases, very small, and often characterized by "digging a few holes in the earth", as a young archaeologist from Deccan College sarcastically noted. Although there were exceptions to these small holes, university excavations were generally small due to the resource crunch and funding problems. Only a couple of departments like the Deccan College or the department at M.S University in Baroda conducted large-scale "ASI style" excavations. Moreover, a number of university-based archaeologists I spoke to complained that it was virtually impossible for them to excavate larger and prestigious sites (like Dholavira, for instance) because the CABA would not give them the license to excavate. A senior university archaeologist explained, "The ASI is a central government organization. They have a lot of power and money. Even if a University department had the organizational caliber to excavate large sites, it is almost impossible to get the license to do so, particularly with prestigious and large Harappan sites like Kaliganban, Dholavira, Rakhigarhi. You think we did not ask for permission to excavate there? A university department can only dig small Harappan villages but only the ASI has the historical

right to dig Harappan cities!" The sarcasm reflected reality; university led excavations over the past fifty years, have only dug Harappan villages like Padri, Rodji, Bagasara, Nageshwar to name a few.

Since my graduate student days at Deccan College, I had always been intrigued by the perceived necessity of large-scale excavation while reading reports of ASI excavations. So, when I began my ethnography, I wanted to discover the rationale underlying this *fetish* and would often question my informants about this. It was during a conversation with a group of Asst. Archaeologists and other staff members, huddled in a canvas tent one wintry night after a sumptuous meal, that I was told the "real reason" for ASI's obsession with large-scale excavation. It did not come as a surprise to be told that the "real reason why ASI does large-scale excavations was not because all these sites needed huge digs. But big excavations, lengthy excavation, large excavations mean money. And that is the real reason sir *ji* [*bada excavation, lamba-samya wala excavation, aur phhela hua excavation ka matablab hai paisa. Yehi hai asli karan, sir ji*]. And once again the connection between corruption and archaeology in the ASI was explained to me. The political economy of a large-scale excavation made it the preferred intervention for ASI archaeologists working in the Excavation Branches. After talking with numerous informants I came to know that Excavation Branches were the least funded of all the units of the ASI. The Circle, was the most heavily funded division of the ASI, where the majority of the funds were allocated to do conservation work. Deputation in the Circle was considered to be the most lucrative of all jobs in the ASI. Numerous informants told me that to be the SA of a Circle was the most "money-making position in the ASI." This was because a huge amount of funds were allocated for conservation work, which provided a "ready-made" setting for pilfering money by either using sub-standard restoration materials or through "kickbacks" involved in procuring materials and supplies for conservation work. On the other hand, the only way of "making money" in the Excavation Branch was to conduct an excavation, and "what better way than having a large-scale excavation, which went on for many years, which required hundreds and hundreds of labor, and tons of material and supplies [*maal-pathar*]."

In a moment of rare self-reflexivity, another informant, an ASA, told me at another time and at another location, while relaxing in a mud hut on a very hot afternoon in Baror: "We all in the Excavation Branches love excavations. We all benefit [*fayada*] from it. Some make huge

amounts of money while others make a small sum of money. But we all benefit from it. From fixing muster rolls to making false receipts for travel, to buying unnecessary supplies. We are all corrupt. But then, you cannot blame anyone, for the whole system is corrupt. Look, when you see everyone in the Circle making money because of the huge Conservation projects, from the SA to the mere peon [*chaprasi*], what do you expect us to do? When you see that a Conservation Asst. drives a *Santro*[63] to work everyday, then what do you do? It becomes difficult when you realize that you have been in the ASI for 15 years and you don't own even a scooter and the Conservation Asst. is just a young lad [*kal ka chokra*]." Such narratives were common throughout my fieldwork experience; they had almost become part of my daily life. So it was not difficult for me to make the connection between the ASI's apparent *fetish* for large-scale excavations and its political economy rather than its epistemic necessity. The total budget of the Saraswati Heritage Project for the year 2003-2004 was Rs 53.35 million and for the year 2004-2005 was Rs. 23.98 million. This included conservation, excavations, and building of heritage complexes. For the year 2003-04, the money allocated for the excavation work at Dholavira was Rs. 6 million, for Baror it was Rs. 4 million, while for Bhirrana and Juni Kuran it was Rs. 2 million. In such a situation, ASI-style large-scale excavations did make a lot economic sense.

In the ASI's institutional imagination, the idea of archaeological excavation was fashioned from both the historical genealogy of its colonial legacy and the organizational power of its statist subjectivity. It was the convergence of these two elements that drove the contemporary self-perception of the ASI archaeologists and made large-scale excavation the normative and customary form of archaeological intervention. For the ASI and its archaeologists who prided themselves as the custodians of the vast wealth of archaeological heritage of India, large-scale excavations seemed only fitting to their status, or as one Site Director matter of fact noted "its natural." Not once during my conversations and interviews with the archaeologists and the staff at various excavation sites, was I given a response that justified the necessity of a large scale excavation as emerging from the subject matter and goals of the research. Large-scale excavation was always framed as a given - the normative and/or naturalistic outcome of the very *being* of the institutional and organizational subjectivity of the ASI. It is clear that this

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[63] Santro was a small car made by the South Korean manufacturer Hyundai that had become a new status symbol for middle class India during the years I was doing my fieldwork. It had replaced the earlier middle class vehicular status symbol - the Maruti car, which had been the preferred car for nuclear middle class Indian form nearly a decade and a half from mid 1980s.

rationalization was superficial and lacked any academic justification; the truth is that these excavations were run on the logic of their political economy.

Horizontal Excavation

The "large-scale" excavations that the ASI conducted were conceived of and carried out using "horizontal excavation" - a technique of excavation chiefly suited for big archaeological sites, which required huge areas to be exposed. In the case of the ASI, horizontal excavation usually consisted of "opening up" a few thousand square meters of earth, exposing multiple trenches which were spread over the immense landscape of the site. Methodologically, unlike the open area excavation, which also involved stripping a large area of the archaeological landscape, horizontal excavation, as practiced by the ASI, employed the Wheelerian grid as the means of controlling and disciplining the knowledge output. It was defined in an ASI Institute textbook as:

> a type of clearing excavation composed of large squares to reveal the horizontal extent of the site while preserving a stratigraphic record in the baulks left between the trenches...In horizontal excavation, the site grid is maintained. The site grid is a set of regularly spaced intersecting north-south and east-west lines, usually marked by stakes (pegs) providing the basic references for recording horizontal co-ordinates within a site (Rajan 2002: 85).

Thus horizontal excavation was basically an open area excavation integrated with the Wheelerian grid in the form of balks for stratigraphic control and recording. Within the ASI, it was viewed as the definitive form of archaeological intervention for large-scale excavation; it was methodical, organized, and scientific. It was also the most expensive, elaborate, and extensive type of archaeological excavation undertaken by the Excavation Branches of the ASI. In fact, in the ASI, even small-scale sites, which involved the opening up of a few trenches, were excavated along the basic principals of horizontal excavations.

The basic principles of horizontal excavation were those of open area excavations. The latter technique was formulated and perfected by early archaeologists like Petire and Wooley in the monumental sites of Abydos and Amarna in Egypt and Ur in Mesopotamia. In India, the genealogy of open area excavation goes back to the early twentieth century when Marshall joined as the DG of the ASI. Early excavations conducted under his tenure by the ASI, especially at the large Early Historical and Buddhist sites of Charsada, Taxila and latter the

Harappan town - Mohenjo Daro, are good examples of open area excavations in India. However the transformation of open area excavation into horizontal excavation took place with the coming of Wheeler. He advocated the necessity of conducting excavations with a stratigraphical matrix for efficient and exact spatial and temporal recoding. The ASI's practice of horizontal excavation in postcolonial India was a faithful continuation of the methodological innovations introduced by Marshall and Wheeler in the technique of archaeological excavation. It incorporated the methodological strategies of large-scale excavation of a village, town or a fortification, introduced by Marshall and concurrently integrated the scientific systematization and organization of the Wheelerian grid.

The ASI archaeologists considered horizontal excavation as the most standard technique of digging archaeological sites. "It is the most appropriate way [*sab se sahi tarika*] of digging a large site in India – Harappan, Early Historic or Medieval,[64]" remarked an ASA. "Wheeler himself advocated the usage of horizontal excavation to dig large sites. The ASI is best at doing this. Any large scale excavation has to be dug in a horizontal manner if you want to know the complete cultural deposit of the site," the ASA continued to explain. The ASI's predisposition to extensive uncovering through horizontal excavation turned the epistemological framework of the Wheelerian grid into a pragmatic tool for ensuring control and discipline of archaeological artifacts, material culture, and the knowledge produced at the excavation site, as well as managerial control over the people who worked at such a large site. Wheeler's Method and the "complete acquisition of knowledge" oriented approach of open excavation were complementary. The horizontal excavation produced a huge amount of material cultural evidence, while the gridded network of Wheeler's system provided a systematic apparatus for the controlling and organizing the evidence produced. By fragmenting the archaeological site into convenient units, Wheeler's method brought about order into a potentially chaotic system of knowledge production (Wheeleer 1954: 80). This method had numerous advantages over the open area excavations. Wheeler, in "Archaeology from the Earth," points out the veritable benefits of the grid while conducting horizontal excavation:

> ...(a) conveniently and clearly sub divided for record and control; (b) capable of easy, progressive expansion in any direction without breaking down or impairing the

[64] Early Historic sites primarily consist of Buddhist sites and sites in Gangetic plains – NBPW sites, Iron Age sites etc. Medieval in the Indian archaeological context refers to Rajput sites, Mughal sites in north India.

preliminary datum-lines; (c) capable of preserving for constant reference at a maximum number of points complete vertical sections until the last phase of the excavations; (d) capable, ultimately, of easy integration into a continuously exposed regional excavation; (e) readily accessible to all points for the removal of soil, without hindrance from intervening cutting or traffic across the excavated surface; and (f) sufficiently open to the sky to ensure the easy inspection of well-lighted sections at all required depths (Wheeler 1954:82)

Thus a horizontal excavation, with the Wheelerian grid, was the most practical way of digging large-scale excavation sites. The monumental nature of the archaeological excavation that the ASI conducted, necessitated the disciplinarian mechanism embodied by the Wheelerian grid. The normative nature of large-scale excavation arose from the practical advantages of the horizontal excavation and the systematicity of Wheelerian grid.

As far as historians of Indian archaeology are concerned, both forms of innovations in archaeological excavation have been hailed as scientific and technological advances in excavation methods (see Chakrabarti 1988: Lahiri 2005; Singh 2004). However, when Wheeler introduced his techniques in Indian archaeology, he famously termed the Indian excavation practices followed prior to his time as chaotic, unsystematic, and reflecting "concentrated confusion" (Wheeler 1954: 80). By the time I was doing my ethnography, horizontal excavation represented the emblematic form of archaeological intervention for the ASI and had been practiced for over half a century, without any technological or technical modification. The ASI archaeologists considered it an efficient form of archaeological intervention incorporating the ideas of two foremost colonial archaeologist-administrators. However, within their genealogical narrative, Marshall's contribution was far overshadowed by Wheeler's innovation to the method. Within the paradigmatic framework of ASI archaeology, horizontal excavation was the synthesis of the historical articluation of open-area excavation, the managerial efficacy of Wheelerian grid, and the normative necessity of large–scale excavation.

Theoretically and methodologically, horizontal excavation contrasted with vertical excavation. Whereas horizontal excavation methodologically focused on exposing the cultural layers of a large area of landscape in order to study the cultural deposit of each period, vertical excavation was primarily a vertical probe into the depth of the earth to study the cultural

history of a site or a mound. The crucial conceptual difference between a vertical and horizontal excavation was that the former had temporal focus whereas the latter had a principally spatial emphasis. Vertical excavation usually involved a ten-meter by ten-meter trench that was laid at the topmost region of the mound and excavated till it reached natural surface. The goal of vertical excavation was principally to uncover the chronological history of the site by correlating the artifacts discovered with the stratigraphical layers of the trench. In contrast, the methodological emphasis of horizontal excavation was to uncover the whole excavation area in order to expose the spatial formation of *one* cultural deposit of the site. The temporal aspect of the site was usually kept fixed and the focus was on uncovering the spatial organization of that particular cultural layer.

In a typical ASI excavation, I observed that both techniques of horizontal and vertical excavation were followed simultaneously. The principles of vertical excavation were employed to decipher the chronological history of the site and horizontal excavation used to expose the most monumental cultural layer of the site. This was exactly as Wheeler had emphasized: "*vertical digging first, horizontal digging afterwards*, [that] must be the rule (italics in the orginal)" (Wheeler 1954: 85). For example, at Dholavira, the cultural layer that was exposed most at the site was the mature Harappan layer, which consisted of monumental architectural remains like the fortification wall and the houses and streets. At Bhirrana and Baror, it was the early and mature Harappan layers consisting of mud brick architecture that were exposed. Although the principal theoretical impetus of the excavations in these sites was horizontal and spatial exposure of the Harappan layer (even at small sites like Chak 86 or Tarkanwala Dera), vertical excavation was employed in various trenches to study the cultural chronology of these sites. The techniques of vertical excavation were employed at the level of trench or quadrant, it was horizontal excavation that structured the archaeological intervention of the whole site. Determining which cultural layers in a horizontal excavation needed spatial exposure as opposed to a vertical probe was a complex task and depended on multiple variables - the research focus of the archaeological excavation, the material culture discovered, the nature of cultural deposits, the availability of labor, supervisors and archaeologists at the sites, and also financial, temporal and social constraints.

Horizontal excavation in the ASI sites was a "structural feature" driven excavation, where the process of exposure of cultural deposits depended on the form and nature of the structures

found. Discovery of structural features underlay the logic of the excavation. The direction of the trenches and the number of quadrants to be dug were determined by the presence and spread of structural features. The emphasis was on uncovering the architectural features of the site – an excavation technique for creating monumental archaeological evidence. For example, at Baror, I observed that more than ten quadrants in a parallel sequence were opened up in the second season of excavation to trace, what the archaeologists assumed to be a mud brick fortification of the Harappan village. At Dholavira, the search for the monumental reservoirs around the citadel fortification had propelled much of the archaeological excavation in the mid-nineties. An AA who had been a student during those years recounted: "when Bisht sir found the rock-cut reservoirs south-east of the citadel, more than twenty trenches were opened up simultaneously. Around six to seven hundred labors worked twenty-four hours a day in three shifts. 2-3 diesel generators were used to light up the whole area at night while the excavation was being conducted. We were so excited that no one slept or ate properly for weeks. And Bisht sir was always in the trenches. We even got him his food in his trench." Similarly, at most ASI excavations, as I will show later in the next chapter, discovery was driven by the idea of the *monument* rather than the structure and *antiquity* rather than artifact. Monumentality and antiquity governed the micro-process of excavation and its logic; the ASI had a fascination for discovering large monuments and objects with antique value. Horizontal excavation facilitated such a conceptual focus and was a technique instrumentally suited to the archaeology that ASI practiced.

The epistemological logic of a horizontal excavation was aimed at producing an *archaeological spatiality* by removing earth to expose and make physically explicit cultural deposits consisting of monumental architecture and other features. Horizontal excavation was not directed towards the cultural sequence within a site but was concerned with the material culture of the whole archaeological area with a fixation on a few specific temporal horizons - in most cases, the most monumental ones. In this light, the aforementioned textbook explained that the fundamental goal of horizontal excavation was to:

> clear the whole cultural deposit of a site. It is recording and removing each archaeological deposit in broadly the reverse order of their deposition. The sequence or stratigraphy of the site is carefully recorded through continuous recording of surfaces of deposits. The size and shape of the area excavated is in no way predetermined by the archaeologist. It will entirely depend on the size and shape of

the site. This method allows the archaeologist to obtain a larger and a more meaningful number of artifacts, architectural features, industrial activities, site structures, and all other human activities at the site (Rajan 2002: 96).

Importantly, horizontal excavation as practiced by the ASI was executed with the aim of not just *accumulating* evidence about an archaeological event but rather that of *creating* evidence about the archaeological event: evidence in the form of houses, streets and fortification, and other monuments that were excavated. This *created evidence* about material culture was elaborately documented by using an array of representational techniques from traditional photography and drawings to more recent videography and digital mapping (which will be the focus in the next chapter of this dissertation). Dholavira and Surkotada, for example, were iconic examples of this *archaeological spatiality as evidence* - shaped and produced by the archaeological intervention of ASI's horizontal excavation. The sites today consist of monumental architectural edifices, fortification walls, house foundations, streets, and drainage systems that were excavated and are now preserved as permanent evidence of Harappan culture[65].

This excavation technique was thus not just a knowledge production practice but a technique centered on creating an epistemic archaeological spatiality as heritage. The most prominent objective of horizontal excavation in the ASI was complete acquisition of knowledge about the most prominent cultural layer of a site and its final display an heritage structure. Sometimes, a site might be displayed for only a short period of time, during which a permanent record of the heritage was recorded and preserved forever via representational systems. In the case of the Saraswati Heritage Project, similar to earlier Harappan archaeological projects, the prime focus was always the Harappan cultural deposit. Most of these sites had multilayered cultural deposits, and in many cases the upper layers consisted of Early Historic structures and cultural deposits. But at each site, the research objective of the excavation was consistently to excavate and expose only the Harappan layers, unless the structures of the upper layers were truly monumental. For example, the Buddhist Stupa at the top of the mound in Mohenjodaro was left intact, or in Dholavira, Chalcolithic circular house

[65] This was also possible because the sites of Dholavira and Surkotada unlike Kalibangan, Baror or Bhirrana, were not mud brick settlements but were cities at the mature Harappan level, made primarily of stone structures, which has been exposed and remains intact.

structures were exposed and left intact on the top of the monumental citadel. In Dholavira, more truly than any other site I worked in, the excavation work was firmly motivated by the need to create a heritage site that was the most monumental among the Harappan (or Saraswati in official parlance, then) sites.

The Mound

It was very late at night when I reached Dholavira after a long journey of more than twenty-six hours in State Transport (Maharashtra and Gujarat) buses and private jeeps. When I reached the excavation camp, I was greeted by the co-director of the site who was sitting in the courtyard of the famed mud hut, dressed in a warm woolen sweater, *monkey-cap* (ski-cap), pajamas, and *Hawaii chappals* (slippers). It was the end of November and the winter was slowly setting into this arid and dry belt of western India, with the nights becoming bitterly cold. He was sitting in a molded plastic chair in the company of his staff members (similarly attired) - the two AAs present at the site, and the storekeeper. The co-director introduced me to the members of his staff, who were crouched over a blue molded plastic table full of papers and notebooks, discussing the logistics of the next day's work. In the course of introductions, he commented that it was a pity that I had entered the site late at night or I would have had my first glimpse of the Dholavira mound from at least a kilometer away. "Dholavira is the second largest Harappan site in India - it is simply monumental. You have probably never seen a Harappan mound like this in your life", he emphatically remarked.[66] During my stay at Dholavira, almost all the archaeologists and the staff members talked about Dholavira as a site with a "stunning" [*dhaansu*][67] mound. Of all the stories that I heard about the monumental visuality of the Dholavira mound, the most fascinating one was about an Indian Army general who was on a scouting trip in this border zone, flying above the site.[68] He forced his entourage of helicopters to land in order to take a whirlwind tour of the site. The AA, recounting this episode to me, noted that the army general had revealed that he had never seen such a large archaeological site in his life. That was why when he caught a glimpse of the site from "above" he had been tempted to land and tour the site on foot.

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[66] Dholavira is the fifth largest Harappan site known, with Rakhigarhi the largest in India. In terms of area, the largest Harappan site is Mohenjo-daro, followed by Harappa, Ganeriwala (yet to be excavated), all situated in Pakistan.

[67] *Dhaansu* was Hindi urban slang which literally meant - huge, stunning, big.

[68] This particular visitation occurred in the 2001-2002 season during the massive build-up of the Indian Army on the border with Pakistan, soon after the attack on the Indian parliament in December 2001, allegedly by Pakistan based Kashmiri insurgents.

At Hansi - a huge mound surrounded by a medieval fortification, the director, while giving me a tour, modestly admitted "I have never dug such a big Harappan mound in my life. This is undoubtedly a challenge" [*hamne toh bhai, jhindigi main kabhi bhi itena bada Harappan mound nahi khoda hai. Yeh ek challenge hai*]. The mound at Hansi was a multicultural site; the habitations deposit was more than thirty meters deep, beginning from the mature Harappan phase right upto a nineteenth century British settlement at the top. The trip to Baror was an overnight journey in a Haryana State Transport bus. The ASA accompanying me to the site had been sitting in the window seat throughout the night. He had a habit of chewing tobacco-laced *paan*, which had to be occasionally spit out of the window after adequate mastication. However in the early morning when dawn broke, he especially woke me up, offered me the window seat, and informed me: "Keep your eyes open, don't doze off. You will see the mound from the highway. It is a very large mound" [*doorse dihk jayega. Bahut bada mound hai*].

Throughout my fieldwork, the emblematic significance of the mound was reiterated at each site that I visited - its nature, its extent, and its depth. Innumerable discussions and lengthy conversations would occur in the confines of cloistered tents or in the expanse of the site about the irresistible presence of the mound, its monumentality, or its destruction through erosion and man-made interference. At the site of Chak 86, the AA who gave me the site tour, bitterly lamented the destruction of the mound. He told me that not only had agricultural intervention decimated part of the site, but recently there had also been a brick kiln that destroyed the majority of archaeological evidence at the site: “they have totally destroyed the mound. It’s a pity, this was a protected site, and they have totally massacred it [*site ko maar dala*]. We are moving the court against the owner, but all is lost [*sab khatam ho gaya hai*]. The habitation deposit is less than a meter. It has been just a month and half of excavation we are already hitting the natural surface. I don’t think we will be coming next summer”. Decidedly so, the Bhuvaneshwar Ex. Br. that was excavating the site did not continue the excavation in the second season of my fieldwork. Such concern about the destruction of the mound was frequent.

During exploratory field surveys, each new site that was visited by the ASI exploration team was measured by the height and the extent of its mound. Often sites would be described in terms of the nature of their mound (big, small, sunken, and scattered) [*bada, chota, daba,*

bikhra] or the number of mounds. In these discussions, the mound was always described within a narrative of visuality and physical appearance. The mound was viewed as the visible symbol of the archaeological possibility that a site possessed. The encounter of the mound - to see it and visualize it as an unexploited archaeological resource was the initial experience through which the ASI engaged with the landscape as an archaeological site. B.K. Thapar, in the Kalibangan Report, cites A. Ghosh's description of the Kalibangan mound:

> The site is by far the largest as yet found in Bikaner. It comprises, in fact, of two independent mounds parallel to each other, with a valley about 61 m between. The mound nearer the road is oblongish, about 457.2 m x 245.4m the area being north to south. The other one is about 45.2 sq. m with sullies and ramifications. Both are about 9.15m high. All indications show them to be of the same age but they were distinguished as KLB-1 and KLB-2 if some differential be found later on. All Harappan objects but only a few seemingly Sarainagar objects were found at the top of KLB-1. An old man informed us that the mound had been excavated for six months, 51 years back for railway ballast" (Lal et al 2003:15)

Such descriptions were mandatory in all the reports of the ASI - typically located at the beginning of the report. The semiotic of such descriptions of the mound was that they metaphorically alluded to the knowledge potentiality of the site. Situating the description at the earlier part of the reportage narrative piqued the readers' curiosity about the impending discovery waiting to be unearthed. This narrative practice was not peculiar to the reports written in the ASI but its genealogy can be traced to the earliest archaeological reports written in the late nineteenth century (Hodder 1989) which in turn were influenced by seventeenth and eighteenth century colonial travel writing and scientific reportage (Pratt 1992)

The physical site where an archaeological excavation was conducted was divided into two conceptual entities, defined by the degree of archaeological intervention. The unexcavated but archaeologically *potent space* was referred to as the 'mound' and the archaeologically *exploited space* was the excavated 'trenches'. A physical area, considered to be archaeological potent, was discovered during the process of exploration. During exploration in the Harappan countryside, archaeologists usually discovered and recognized the mound as an archaeological resource, labeled it as a possible site. As explained in the earlier chapter, exploration was one of the elementary ways of bringing new sites into the purview of archaeological knowledge. Almost all archaeologists working in the ASI had conducted

exploration work at some time in their careers to discover new sites and bring them into the fold of the discursive grasp of South Asian archaeology. In the case of Harappan archaeology, since the days of Aurel Stein in the beginning of the twentieth century and after partition with the work of A. K Ghosh, exploration had been an acknowledged disciplinarian practice for reporting new Harappan sites (Stein 1943; 1942; Ghosh 1956; 1959).[69] Generally these spaces in the Harappan region were topographically at a higher elevation than the surrounding landscape and characteristically termed "the mound". The archaeological potentiality of the site was not determined only by the height of the mound but also by the nature and amount of archaeological material culture observed during the preliminary surface exploration. The number of ceramic fragments determined the potency of the site along with the presence of artifacts - microlithics, brick rubble, shell-bangle pieces, and in rare cases fragments of terracotta figurines, or even rarer Harappan seals. These artifacts would be strewn throughout the landscape designated as the site. The cultural categorization of the site was determined mainly by the ceramic type and quality. Within the scholarship on Harappan archaeology, the existing typologies and categorization of cultural material, particularly ceramics, were considered by the ASI archaeologists to be beyond dispute and described as canonical in textbooks.

The mound held a special place, as it was the "bulge" in the landscape that had the potentiality of architectural remains and structures to be found. Jagat Pati Joshi, writing about the Surkotada mound in his site report, explicitly frames the archaeological possibility of the mound within a narrative of potency: "At the time of its discovery, the mound at Surkotada appeared to be a potential site with not only its rubble fortification exposed at places on the surface itself but also having an adjacent lower area yielding Harappan and other pottery and antiquities" (Joshi 1990: 14).[70] In the site reports of the ASI, the mound was always framed within the narrative of potentiality and possibility. Almost all site reports of the ASI began

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[69] Alexander Cunningham was one of the earliest to survey the Harappan countryside. Other prominent explorers include Luigi Tessitori, R.D Banerjee, and Daya Ram Sahni (Cunningham 1875; 1877; Tessitori 1919; Banerjee 1924; Sahni 1924; 1925). For a historical narrative about the prehistory of the discovery of the Harappan Civilization, see Lahiri 2005.

[70] J.P Joshi, describing the mound, also notes: "The mound has an average height of five-to-eight meters (east-to-west) and was discovered by the author during the course of his exploration in Kutch in December 1964. It is higher on the western side and lower on the eastern side. The length of the mound from north to south, including the lower portion, is 160 meters while its width is 125 meters" (Joshi 1990: 15).

with a description of the mound, a tradition followed since the earliest site report written by Alexander Cunningham (Chakrabarti 1988: 59). In these site reports, the mound was represented as the physical embodiment of archaeological knowledge, waiting to be excavated. It was a metaphor for the archaeological knowledge hidden in the depths and crevices of the landscape as well as a physical marker of the site's archaeological potentiality.

Wheeler, famously describing his first brush with the archaeological mound at Harappa, recounts in his typical hyperbolic rhetoric: "Next morning, punctually at 5.30, our little procession started out towards the heaps. Within minutes, I stopped and rubbed my eyes as I gazed upon the tallest mound, scarcely trusting my vision. Six hours later my embarrassing staff and I were still toiling with picks and knives under the blazing sun, the mad sahib setting a relentless pace." (Wheeler 1956: 190). The mound had a special semiotic valence within the context of South Asian archeology - it represented an epistemologically persuasive space that necessitated archaeological intervention. The mound was a physical space that *had to* be archaeologically "unearthed and excavated" [*khodana padega*]. As Wheeler further narrates: "As I approached the highest mound at Harappa on that May morning, the truth, or a part of the truth, of the matter stood suddenly revealed to me in a the strong slopping light of the early sun...[T]here remained the task of demonstrating the structural make-up of the newly found acropolis by excavation" (Wheeler 1956: 191-92)[71]. The size of the mound, as Wheeler categorically observed, was a direct measure of the archaeological wealth that was located at the site. It was a visual correspondence to the importance of the archaeological intervention.

Since the days of Marshall (and Wheeler) - large-scale excavation had been the norm in the ASI, and the size of the mounds dictated the magnitude of archaeological activity conducted at the site. I was often told by archaeologists in the ASI that there was prestige associated with excavating huge archaeological sites. An AA poignantly commented: "big archaeologists dig big mounds; small archaeologists dig small mounds; and Assistant Archaeologists (AAs) dig nothing" [*bada archaeologist bada mound khodtha hai; chota archaeologist chota mound khodtha hai; aur Assistant archaeologist kuch nahi khodtha hai*], lucidly capturing the

[71] About the mound of Mohenjo-daro, Wheeler notes: "There, as we drove through the heaps and scrub towards the heart of the site, a high mound rose suddenly in front of us, crowned with the tattered *stupa* or shrine" (Wheeler 1956: 191-92)

proportional relationship between archaeological mounds and the ASI hierarchy.[72] The physicality of the mound and its visual presence was a powerful subtext in the formation of the archaeological imagination among the ASI staff that worked at the site on an everyday basis. The measure of the mound was the embodiment of the meaning, direction, and justification for the archaeological intervention for the ASI archaeologists.

The size of the site was often driven by the physical presence of the mound. In the sites that I did work in, except for Dholavira, the extent of the mound determined the boundary of the site. Even at Dholavira, the majority of the excavation was conducted on the numerous mounds, although there were some attempts at moving beyond the mound. As an AA explained to me: "The mound was the site, and the site was the mound. There is no interest [*iccha*] in looking at the large settlement pattern, neither is there an interest in doing a site catchments analysis. The excavation site in the ASI archaeology was centered [*kendrit*] on the mound. This is because the mound is where most structures are usually located". This was indeed the case in the SHP sites like Baror, Bhirrana, and Hansi, where excavation was conducted only on the mound. Rarely during my fieldwork did I see any attempt at excavation beyond the extent of the site-mound. The director at Bhirrana explained, "We have very little time, usually only three to four seasons to excavate. And these mounds, as you can see yourself, are so huge that it does not make much sense to do any excavation work beyond the mound. Only at Dholavira, excavation is conducted beyond the mound. But remember, Dholavira has seen more than ten seasons of excavation." The power of the mound was so irresistible that the ASI archaeologists usually did not make any attempt to excavate beyond its confines. The mound with its monumental presence framed the archaeological research agenda because the potentiality of excavating large structures under the mound and exposing them was very real and tempting. The mound was the centre of archeological activity in any Harappan or early historic site of the ASI, and the goal of the survey work was to transform the monumentality of the mound into an epistemic spatiality fit for the discovery of the past.

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[72] In the ASI only officers of the rank Dy. SA and above were allowed to apply for license to excavate an archaeological site. This discrimination caused demoralization among the archaeologists of the AA rank. They complained that in a University setting, a mere Lecturer can apply for license to excavate while they could not do so even if they had years of excavation experience and PhD degrees in tow.

Survey-work

"How can excavation be possible without a survey of the site? You cannot dig wherever you like. It has to be done properly" [*sahi tarike se karna padega*], noted a surveyor of the Patna Ex. Br at Baror, while he was lecturing a group of students from the Institute, about survey techniques and the importance of survey in archaeological excavation. We were all standing at the topmost part of the mound, surrounding an optical theodolite, as the surveyor was explaining the importance of survey work. The "work" [*kaam*] of the surveying team, he explained to the students: "begins much before the excavation starts or even before the camp is set up." The task of the surveyor, he described, consisted of "first finding the topmost part of the mound, noting its elevation; positioning the exact coordinates of the extent of the site; mapping the site; creating a topographic map; gridding it; and then doing the layout of the trenches. This is survey-work" As he was explaining, one student after another was peering through the telescope of the optical theodolite. Each student was trying to note the marking on the three meter tall wooden collapsible scale held by a laborer about seventy meters away, at the edge of an excavated trench. The survey team usually consisted of a surveyor, a draughtsman, and often two to three laborers who would help in moving the scale, using the measuring tape, hammering the trench peg, tying strings, or simply carrying the theodolite or the tripod.

In the ASI, "survey-work" was divided into two kinds of practices. Wheeler explicates the task of the surveyor and the surveying team: "(a) preparation of the site-plan, general contoured and often of considerable extent; and (b) the planning of a structure or group of structures on a small scale or in detail. For (a), either a plane-table or a theodolite may be used; (b) is normally carried out by triangulations from a base line" (Wheeler 1654:167). Thus the first was the process through which a known, unexcavated site was scientifically rationalized, and prepared for excavation: "through survey work we make the site ready for excavation" [*survey kar ke ham site ko excavation ke liye* ready *karthe hai*], explained the draughtsman to me. The operative word here is the English word "ready", which referred to the task of situating in the landscape an area deemed fit for archaeological excavation (usually the mound), as contrasted with the campsite or the pottery yard, which also came in the ambit of the archaeological site. The second type of practice occurred after the excavation had begun and structures and artifacts were being unearthed. During this period, survey-work involved the practice of

mapping each trench or a group of trenches, at a particular level where the archaeologists had deemed it fit to stop the excavation and start mapping of the layer. In this section, I will talk about the first kind of survey-work conducted by the reconnaissance team that visited the designated archaeological site before the excavation, to make the site fit for archaeological excavation.

The Baror and the Bhirrana reconnaissance teams visited the site before the excavation team arrived to conduct a survey of the site. Once the sites were identified, the preliminary survey included the identification of archaeologically rich mound(s).[73] A reconnaissance team consisting of the surveyor, a draughtsman, a photographer, a couple of Asst. Archaeologists, along with class four employees, and sometimes the SA of the excavation branch conducted this preliminary survey. This preliminary survey was usually done a season in advance, "if the SA was academically oriented," I was told by an ASA wryly, "but how many academically inclined archaeologists are there in the ASI? Just a handful [*muthi bhar*]." In most cases, survey-work was undertaken during the summer before the excavation began, and lasted only a few days. "Earlier archaeologists were scholars who would explore an area and then decide to excavate. But nowadays excavation has become yet another bureaucratic task [*sarkari kaam*]. If there is a site, then let's dig [sit*e hai, to khodenge*]", continued the ASA. Survey-work was a standard archaeological protocol that had to be followed before any site was excavated, however superficially it was carried out. The "academically oriented" archaeologists of the ASI would conduct a detailed survey of the site before excavation commenced but at most SHP sites, excavation was preceded only by the barest minimum of survey-work. In the case of Dholavira, the co-director of the site informed me that Dr. R.S. Bisht had conducted two years of extensive exploration and survey-work at the site before he started digging.[74] On the other hand, in the case of the SHP, the reconnaissance team of the Patna, Baroda, Nagpur, and Bhuvaneshwar Excavation Branches were sent just weeks before the excavation had been planned in November 2003. An AA, criticizing this short notice, explained, "we were just ordered to go and dig, there was no planning, no research agenda. Just go and dig" [*hamare uppar site thopi gayi thi. Jao, bus khodo*]. During their

[73] Large sites like Dholavira and Rakhigarhi were huge sites with numerous mounds
[74] I came to know later, however, while talking to an AA, that the reason that Dr. Bisht did two years of exploration work was because he had not been granted permission by CABE to excavate the site as other senior ASI archaeologists at that time (1989-91) were also interested in digging the site. It was well known by then that Dholavira was one of the largest of the Harappan urban settlement in India and was monumental in proportion.

reconnaissance trip, the team camped at the government guesthouse [*Dak-Bungalow*], made preliminary contact with district officials - the District collector, the block officers, to begin the process of obtaining permission for the excavation. It was also at this time that preliminary contact was established with the local inhabitants to organize the logistics for the setting up of the archaeological camp and importantly, to source out the prospective labor to work at the site. The SA and the AAs did this contact work while the technical staff of the Branch camped at the site, taking photographs and conducting survey.

According to the textbooks that were read by Institute students, the purpose of the archaeological survey was "to locate the site; to determine site boundaries; to completely plot the area; to establish the trench or grid pattern; to describe the topographic nature; to measure the site; to locate the findspots with the site; to prepare the composite map of the area" (Rajan 2002: 219). However the "survey-work" at this preliminary stage only included a "*land survey* and a *topographical survey*", explained a surveyor at Bhirrana, "We try to do both, but that is never possible"(*koshish rahti hai per kabhi hota nahi hai*). Upon being asked why both surveys were not possible, he explained, "Because the archaeologists [*saheb log*] are in a hurry to dig. They want us to give them the site-grid so that the trench layout can be done. That's it [*bus*]. But topo-survey is usually [*jyadatar*] done simultaneously with the excavation work". This survey, according to the textbook, was defined as consisting of "the establishment of a site datum point, the datum line, the base line, and the position of the overall site grid for proper planning and laying out of the site" (Rajan 2002: 225), whereas a topographical survey "should be made to show the contours of the site and the natural features of the area so that the interpretation of the site will include an understanding of its environment" (Rajan 2002: 225). Other than at Dholavira, this survey-work ended in less than a week, during which the extent of the site was demarcated and the top of the mound was identified. At Bhirrana and Baror, while talking to the surveyor, I discovered that due to the paucity of time during the reconnaissance survey a very preliminary map had been made- "a rough map" as one of them explained. At these new SHP sites, detailed topographical mapping and excavation work was conducted simultaneously with the excavation of the site. During the reconnaissance survey, other objectives included cordoning off the site and installing a signboard, which declared the site as a protected monument.

The first task of the surveyors, during their reconnaissance trip at the site, was to locate the elevation of the site from the mean sea level followed by the location of the site datum points. These points was crucial in laying out the Wheelerian grid, as a textbook explained:

> In any archaeological investigation, the survey or laying a site grid starts with a permanent control point called the *site datum point*. This point has to be well established as it serves as a reference point in all future works at the site...There are two methods in establishing site datum points. One is the central datum and another is the off-site datum. The central datum is almost located at the centre of the site from which the grid originates. The off-site datum is placed outside the perimeter of the site, usually on the southwest part of the site (Rajan 2002: 227)

The process of locating the elevation alongwith the two datum points was a complicated one, especially without the help of GPS or any other satellite based cartographical instrument, which were not commonly used by the surveyors of the ASI during the years I was doing my ethnography. I learned that the elevation from the mean sea level at the sites of Baror and Bhirrana had been calculated from the benchmark elevation at the local railway station. But in the case of Dholavira, I was told that topographical maps of the area were not available to the public as this was a border zone and the Indian defense authority had restricted the public distribution of topo-sheets of most districts adjoining the Indo-Pak border. Therefore, at Dholavira, the surveyor had to calculate the site elevation from the actual sea level, which was around four kilometers away from the site. This was an arduous task without the help of any electronic and digital equipment and was done using an optical theodolite by "taking and transferring the elevation from one point to another, till we reached the highest part of the mound." Once the elevation was determined, the task of mapping the site was initiated, in order to locate the physical extent of the site. This was done by walking all over the site and locating the number of ceramic fragments spread over the site. It was once the extent of the site was mapped that the datum point could be located.

At the moment of discovery, it was acknowledged that a particular region on the landscape had epistemological potentiality and that archaeological intervention was necessary. This was recognized during exploration of the landscape, while accumulating surface material cultural remains, categorizing them, and locating the ceramics and artifacts found within the known typology of Harappan sites and cultures. However immediate excavation was not possible. The site was not yet prepared for an archaeological excavation, as the survey of the site had

not yet been done. It is only after the survey that the landscape was legible as an epistemological site. Only after the space had been conceptually rationalized within the articulation of the Cartesian framework, was the area with knowledge potentiality ready for archaeological intervention. The survey was a scientific practice of domesticating the wild unknowable landscape into a legible representable universe. It involved encapsulating the archaeological site from a mass of landscape with scattered material culture remains into a representational system, which had a fixed matrix and absolute coordinates. The survey was a process through which archaeologists orchestrated the conceptual apparatus of encompassing a landscape within a Cartesian framework, by fixing the co-ordinates of the site within a two dimensional representational system. It was a cartographic exercise of converting an area on a landscape into an epistemological entity fit for excavation. The act of the survey was the fundamental analytical framework through which the Cartesian impetus of Wheeler's method was imposed on the landscape.

The ASI's *survey-work* was a pre-excavation assessment of the landscape in order to rationalize the terrain into the familiar reference of the disciplinarian discourse of a scientific practice. The archaeologists working at the SHP sites were professional scientists who were given a landscape known to be archaeologically potent to reconfigure it into an epistemological site capable of producing knowledge. As Latour remarks about soil scientists, in his succinct ethnography of field scientists working in the Brazalian Savanna: "Yes, scientists master the world, but only if the world comes to them in the form of two-dimensional, superposable, combinable inscription" (Latour 1999:29). The ASI archaeologists employed the cartographical apparatus for representing the landscape not solely in order to portray the physicality of the landscape but more in order to transform it into a material reproduction of their own conceptual framework (see Duncan 1999; Mitchell 1991; Sluyter 1999; 2001; 2002). Similarly, for the archaeologist confronted with a mass of land with potential, the only way through which creditable knowledge could be produced was to rationalize it into a familiar conceptual universe. The act of surveying the landscape was a way of using epistemologies to reconstruct the physical spatiality into a visualized representation through which the world could be engaged with through one's own conceptual vocabulary.

Laying out Trenches and Quadrants

The intellectual genealogy of the Wheelerian grid network has often been allocated to Descartes and his Cartesian perspectivalism. However, the coordinates system predates him by a few centuries (Berggren & Jones 2000; Kostof 1991). Paradoxically, the earliest example of gridded spatiality has been discovered in the Indus civilization. The gridded cityscape of Mohenjo Daro, (which was subsequently also discovered at other Harappan sites), where the streets intersected at right angles and divided the Harappan towns into residential blocks, have been suggested by historians as the earliest example of grid networks (Stanislawski, 1946: 108-110). This irony of the *double inscription* was not lost to me when, during my first site visit at Dholavira, I was taken to the Lower Town and showed the famous right-angled street intersections that were discovered through the extractive application of the Wheeler Method in the 1990s (see Bisht 1989; 1990; 2000).

The employment of one gridded conceptual apparatus to unearth another gridded spatiality was not without ideological roots. Its operational mechanism was located at the heart of modernity's fetish with acquisition of objective data and production of totalitarian knowledge. The application of Cartesian grids on virgin wildness was by no means Wheeler's innovation, but emerged from a long history of privileging geometrical uniformity as the preferred way for modernity to engage with the physicality of landscape. This imposition of the grid was an analytical export from the cartographic framework of Cartesian perspectivalism into the spatiality of the physical landscape. It was also an ideological apparatus to make the unknowable territory commensurable within the configurations of modernity's own epistemological universe. It personified modernity's desire for legibility, organization, and rationality and has been viewed by scholars of space and spatiality as the "hallmark of modernity" (Dimendberg 1998:2; Krauss 1985; 10-2). It was modernity's instrumental apparatus to make unknown territory commensurable within its own universe of comprehensible configurations. The "geo-power" of the Cartesian grid emerges from its employment in ordering space and in the production of geographic knowledge, both of which tasks were central to the formation of modern governmentality (O Tuathail 1996).[75] Rose-Redwood shows that the grid functioned as a mechanism of disciplinary power. Employing

[75] Ó Tuathail defines geo-power as "the functioning of geographical knowledge not as an innocent body of knowledge and learning but as an ensemble of technologies of power concerned with the governmental production and management of territorial space" (Ó Tuathail 1996: 7)

Latour and Foucault, he argues the Cartesian grid was instrumental in producing a "spatial regime of inscription", which in turn produced a materialized epistemic space epitomized by the Wheelerian grid (Rose-Redwood 2006).

The process of laying the Wheelerian grid on the archaeological landscape did not begin in the field but was first administered on the map in the office of the ASI Excavation Branch. It was only after the extent of the site had been identified as an area productive for archaeological intervention, demarcated both on the physical landscape and the cartographic map, that the first step of the Wheeler Method was set in motion. Before the landscape was inscribed with the "Wheelerian grid," it was outlined on the "survey-map" of the landscape. The survey-map was the cartographical representation of the archaeological site, which was prepared by the surveyor in consultation with the archaeologists who decided the potential coverage of the archaeological site and demarcated it. This map was a normalizing device, which assisted archaeologists in the production of the abstract space that they were most familiar with. The surveyor at Bhirrana explained, "Once the survey map was ready, with its coordinates, elevation, datum points, and extent set, our work in the field was completed. Then we went back to our office desk in the Branch office. There we did the rest of the work. We marked the location of the base line and then divided the map into a 10 meter by 10 meter grid and numbered the trenches according to the quadrant system." The end product of a pre-excavation survey-work was the survey map - the cartographical representation of the archaeological site that rendered the field portable so that it could be carried to the office. The archaeological site had to be first conceptually confined within a two-dimensional cartographical representation system and then fragmented via the Cartesian grid on the landscape. The excavation could not proceed before the landscape to be excavated had been domesticated within a two dimensional representation.

Thus it was within the walls of the Excavation Branch office that the apparatus of the Wheeler Method was first employed, and not on the field. The baseline was first drawn on the map and then subsequently located on the landscape when the excavation began. Similarly the quadrants were drawn with the datum points as the centre of the site on the map, and then the Cartesian grids were outlined on the map. This process has been described in ASI textbooks as follows:

> The excavation grid or trench must be established in checkerboard fashion. A grid

> usually serves as a prelude for starting a large area excavation. As noted above, for laying a grid one has to establish the datum line and base line with terminal pegs at each end. These lines fall at right angles. The terminal pegs should be established outside the excavation area so that these reference pegs would not be disturbed with the expansion. The overall site grid is divided into smaller grids, which are parallel to the site base (east-west line / latitude) and the datum line (north south line / longitude / true meridian) and each subsidiary pegs fall at right angles to each other. This way grid system in the horizontal; plane can be established (Rajan 2002: 86)

All ASI excavation reports since independence had numerous pictures of archaeological sites divided into these Cartesian grids and these grids had become synonymous to archaeological excavation as a scientific enterprise in India. Its symbolic valence was located not just in the pragmatic methodological possibilities offered, which allowed for controlled and disciplined excavation, but also in its affective power to transform the rugged, dusty, and wild landscape into a scientific and orderly one. The visual impact of a landscape divided into clear-cut squares divided by slender balks gave an aura of scientific activity, accentuated by the obvious manifestation of symmetry and balance.

The intellectual affect of the Cartesian grid epitomized the epistemological bulwark of scientific rationality (Motz & Weaver 1993; Akkerman 1998) because the grid was not just a cartographic lens to organize an unknown landscape, but it was also an epistemic framework for ordering and disciplining an undomesticated spatiality by rationalizing it within a scientific discourse. Its imposition produced an abstract representational space, with not only a distinct symbolic system dictating lived experience but also a strong ontological impact (Lefebvre 1991 [1974]). The ontological persuasion of the Cartesian grid was obvious in the street network of modern cites, arrangement of chairs and desk in a classroom, military formations, and other form of disciplinarian spaces (Rose-Redwood 2006). An abstract spatiality that was constructed through the large-scale imposition of the Cartesian grid created a mechanism of disciplinarian power, which, in Foucaldian terms, could be argued as producing a "domain of objects and ritual of truth (Foucault 1995: 194). The Cartesian grid was modernity's fetish with legibility and its desire to fervently grasp the physical world producing a rationalized subjectivity, which rendered its worldview 'normal', 'neutral' and 'objective'. However scholars have argued and demonstrated that this normalizing gaze of the Cartesian grid was located in its usage as disciplinary formation and an organizational schematization for

capitalist comodification of the physical landscape (Harvey, 1990; Scott, 1998; Pickles, 2004).

Colonial scholar-bureaucrats like Wheeler were drawn towards the Cartesian grid not only because it provided control and a coherent epistemic universe but also because the Cartesian grid provided an administrative rationality that was essential to the spatial ordering of the everyday life of the people producing the knowledge. Analogous to Foucault's critique of the technology of surveillance personified by the panopticon and its encompassing gaze, the Cartesian grid has been labeled a repressive tool of social control by Michel de Certeau (de Certeau 1984).

The logic of the Cartesian grid consisted of two standard principles – rectilinear geometry and sequential numbering -- which were instrumental in the production of an abstract spatialized universe – ordered, coherent, and legible (Rose-Redwood 2006: 8). The grid was a product of rectilinear geometric formation based upon the principles of orthogonal order and replication. Fundamental to this formation was the co-ordinate system, which consisted of two central baselines (axes), where each baseline was numbered sequentially thereby forming the grid co-ordinates.

> [G]eometry requires a two-dimensional manifold so that describing the position of any point on this manifold requires two numbers or, put differently, two coordinates. Descartes began by laying off two mutually perpendicular lines on the plane - one horizontal and the other vertical - and defining the point 0 of intersection of these two lines as the "origin" of the coordinate system. He called the two intersecting lines the coordinate axes of his coordinate system, which, since then, has been called a "rectangular" (right angle) Cartesian coordinate system. The position of any point on the plane is specified in this kind of coordinate system by assigning two numbers to the point, one of which is its perpendicular distance, on an arbitrary scale, from one of the perpendicular axes and the other of which is its perpendicular distance from the other axis. Descartes designated the horizontal axis as the x axis . . . and the vertical axis as the y axis . . . The position of a point in such a coordinate system is then specified by its two coordinates x and y, written as (x,y) (Motz and Weaver 1993).

This geometric and mathematical innovation gave the Cartesian grid an aura of neutrality and it became associated with the nineteenth century emergence of large-scale scientific and engineering projects and in the creation of a "geo-coded world" (Pickles 2004). On this were

imposed the sequentially numbered spatial regimes of inscription that had an algebraic basis. This process of first fragmenting the physical landscape within a geometric spatiality and then naming the units using a comprehensible matrix of inscription, was instrumental in restructuring the landscape into a materialized epistemic entity.

Once the trenches were laid out on the abstract spatiality of a map, they were not immediately inscribed on the archaeological site. With the map in the hand, the archaeologists would descend on the site, with hordes of laborers and the first task was to clear up the site. This process involved the removal of natural habitation off the site. "Since the excavation begins in the winter months after the monsoon, the sites are usually full of shrubs, bushes, and occasionally a tree, all of which have to be removed", explained an AA. "Once the site has been cleared, the trenches are laid out". It is the SA along with other archaeologists who determined the location of the first trenches. The survey team would be instructed about the location and they would then inscribe the landscape with the first of the Wheelerian grids:

> "A grid with a unit of 2, 5 or 10 sq m can be established with the help of instruments like Plane Table or Dumpy level. One has to start the work from an already recognized site datum point. First centre the instrument over the datum point using a plumb bob attached to the end of the tripod column. Level it at a convenient height using the built-in-spirit level. Make the north-south line by placing the dumpy level reading to 0 degrees. Turn the level to 90 degrees i.e., perpendicular to the north-south line and make the east west line. The north-south line is called meridian line or datum line. The east-west line is known as base line. One may also orient the level to 180 degrees and 270 degrees to get the further lines to south of datum line and base line to the west of the datum point. In this way, complete datum line and drive a peg at each unit points 5, 10, 15, 20m... distance from the central datum point. Turn the tape along the base line and repeat the same thing. The square grid would be formed once the peg/stake is derived at regular intervals. Then each unit/trench are may be labeled accordingly...These 5 sq. m. trenches are left with an earth portion of 25cm on all its four sides thereby the actual digging area measures 4.50 x 4.50 m. the gap between the two adjacent digging areas measure to 50 cm. This 50 cm wide earth portion is called balk. These are left standing as long as that serves a purpose in the excavation (Rajan 2002: 227-229).

This was the practice every time an excavation trench had to be marked on the landscape

before the excavation. This process was considered to be the job of the survey team, which would be summoned to mark the trenches and the quadrants on the landscape along with their paraphernalia – wooden pegs, 3-inch iron nails, a wooden hammer, small iron hammer, a 5-meter tape, a 50-meter tape, cotton strings, nylon wires, and the survey map. Of the most crucial tools was a photocopied survey map of the excavation site, with the Wheeleerian grid laid out. This map was a conceptual apparatus that was very essential for the archaeologists, as I observed at the site. Before any trench was outlined on the site, the surveyor with his survey-map would be seen peering at the map on the field table and instructing labors to nail the pegs at the points marked on the map. Throughout the archaeological process, the survey-map was the conduit through which the excavation proceeded. It was a scientific artifact that was constantly being remade and reworked as the trenches expanded both horizontal and vertically.

For Wheeler the grid was the most pragmatic conceptual apparatus for inscribing the landscape in order to excavate large sites, as the grid produced squares that were functional units of archaeological excavation:

> "The individual square is a clearly defined sub-unit for record and supervision; supplementary squares can be added in any direction in accordance with developing needs, without affecting any previous datum; the supervisor retains in each square (until the end of the work) a complete section on all four sides of him, together with such additional sections or part-sections as he may care to add within the compass of the square; the stratification of adjacent squares, and therefore accumulatively of the whole site, can easily be correlated and recorded along a number of arterial lines, so that ultimately the barriers between the squares can be removed without loss of vertical evidence and the whole plan laid bare, level by level" (Wheeler 1954: 83).

So, in effect, Wheeler's technique was geared towards transforming the landscape into multiple scientific laboratories, small cubicles driven into the earth, enclosed by the four walls of the balks, which controlled and disciplined the task of excavation. This ordered, coherent, and organized three-dimensional spatial mimesis of the two-dimensional cartographic constitution of the survey map simulated the disciplinarian ontology that was necessary for epistemological action. This reorganization of the landscape into a disciplined scientific space was the most appealing aspect of the Wheeler Method (or its variants) for archaeologists throughout the world who adopted it. As Wheeler emphasizes: "Be it repeated that a great

merit of the 'square' method is that it localizes both control and record. The supervisor's responsibilities are clearly defined, and the area covered by his field note-book is precise. The basis of his record is the careful identification, embodied in an accurate measured drawing of the stratigraphy of each of the four sides of his square and of such supplementary sections as may be required" (Wheeler 1954: 86). Thus, the square became the ontological embodiment of the gridded universe of the laboratory, where scientific knowledge was produced through the articulation of a disciplinarian universe. The innovative possibility of the Wheeler Method was to discard the earlier practice of arbitrary excavation. Although far removed from the treasure-hunting digging of antiquarian archaeologists, the earlier excavation practice had still involved arbitrary unearthing to recover structural or material remains. Before Wheeler's intervention, Indian archaeology had been of this kind. Wheeler provided a refreshing alternative of engaging with an archaeologically potent landscape, an alternative, which had the *correct* mix of discipline - both an ontological and epistemological visual and aesthetic appeal.

Crucial to Wheeler's method was not just the practice of dividing the earth into a gridded spatiality, but also the practice of naming the grids. This was necessary in order to control the chaos of the excavation process, especially in terms of the knowledge produced. This naming was seen by the practitioners of the Wheeler Method as an equally important task because it facilitated a record of the locations of the found artifacts and a description of the excavated landscape. The gridded squares of the excavation site were sequentially numbered, and each artifact excavated was inscribed with the grid number in which they were discovered. For the Cartesian grid to have any scientific value, it had to be numerically coded, as Wheeler explained:

> The squares thus pegged are conveniently named by means of letters in one direction (say, east to west) and by numbers in the other direction (say, north to south). They will thus be known individually as A1, A2, A3, &c.; B1, B2, &c...Thus a peg set at the junction of four squares will have a different designation on each face; e.g. A1, A2, B1, B2. The need for clear and abundant labelling cannot be over-emphasized if error is to be eliminated from the records, particularly on a large excavation (Wheeler 1954: 84)

The pattern of naming described above was the default framework of sequential numbering employed in the ASI sites, with minor variations. The practice of naming was essential in the reconfiguring of the landscape within a knowable practice and its transformation into

epistemic space. This process of naming, exactly like the gridding, was first executed on the survey map in the office of the Excavation Branch and not in the field. In the field, the trench supervisor was given the name of his quadrant, when the survey team pegged the trenches. The importance of the grid numbers was underscored during introductory lectures to the students, by the ASI archaeologists: "you have to be very careful as this is how we will know while writing the report, what was what and what was where [*kya kya tha, aur kya kidar tha*]." Therefore the trench numbers were also inscribed in bold fonts on the site notebook that each site supervisor was given. The numbering, like the gridding, enabled the Cartesian grid to function efficiently as a knowledge extraction and categorization procedure. This was a characteristic process for the production of an abstract spatiality that reduced qualitative attributes to a numerically coded quantitative sign system, which Lefebvre has argued to be one of the distinguishing features of modern capitalist modes of production (Lefebvre 1991).

Latour argues that such an inscriptive practice is central to the scientific project and theoretically refers to all types of transformative practice through which an object became an epistemological entity (Latour 1987; 1999). In his ethnography of geologists in the Brazilian Savannah forest, he shows that such an inscriptive universe of naming is essential for recognition and subsequent categorization of the knowledge produced. He argues that if such an inscriptive universe is destabilized in any manner, then the scientific project completely loses its capacity to produce knowledge.

This notion of a damaged inscriptive universe and its adverse impact on the knowledge production capacity of the archaeological project is exemplified in this example from the Dholavira excavation, as an AA informed me. One evening, I was in his tent, chatting with him, as he was recording the artifacts discovered in the antiquity register, sitting crossed legged on his foldable metal cot. Steatite micro-beads, fragments of terracotta figurines, lapis lazuli, and carnelian beads, neatly stored in plastic Ziploc bags with labels were scattered all over his bed as he was meticulously copying the details of the labels into the antiquity register. In the shadow of the flickering bulb and in the background of the humming diesel generator, he told me in a low tone that the trench numbers and stratigraphical measurements on innumerable artifacts that were discovered in the early years of Dholavira excavation, had been obliterated, due to poor storage: "those artifacts, mostly ceramics and non-precious metal, steatite and terracotta antiquities are of no use. They are now without context. They

would have been essential to construct a deep cultural history of the site. I don't know how it happened; in some trunks the termites have eaten the site-cards and in others the pottery bags have been destroyed. We have totally lost them. They are all trash now [*ab yeh sab kura ho gaya hai*]." Latour belabors this point and says unambiguously that if such a destabilization of the inscriptive universe occurs then "the scientist would be lost in the landscape" (Latour 1999: 29). In the context of Dholavira specifically, and generally in the case of the archaeological excavation of the ASI, such obliteration of the inscriptive universe was a constant problem, as I was told by a number of my informants. Some of the ASI archaeologists admitted that this problem was one factor resulting in the *non-publication* of more than a hundred archaeological reports of the sites that the ASI had excavated since 1947.

The fascination of the ASI archaeologists with Wheeler's Method was not only because this method was considered to be a tradition to be followed rigorously, or the fact that the archaeologists had been trained in this method in the Institute and in the field, but as the Director of the Baror excavation emphatically stated "it is the neatness of the Wheeler Method that makes it popular with the ASI. If you dig large sites like Dholavira or Rakhigarhi, or the massive early historic sites, where we have to expose large areas and undertake horizontal excavation to understand the cultural sequence of the site, the structural nature of the site, the Wheeler method is essential. It is the best and the most organized method. And it looks nice and clean". This perception of *visual* neatness, cleanliness, and organization in Wheeler's Method was not surprising. Even for Wheeler, the layout of the excavation trenches was not just one of the significant processes through which the landscape was rationalized into the disciplinarian discourse of the archaeological enterprise in order to control the production and generation of archaeological knowledge. It was also an aesthetic project of creating a neat and tidy epistemological spatiality.

When he came to India in 1944 he was very skeptical of the utility of the excavation process then in practice. Commenting on a photograph of an excavation conducted in Harappa by Madho Sarup Vats in the 1930s, Wheeler famously notes: "a mere novice might guess, and guess correctly, that chaos reigns. Look at the crowded workmen, picking and shoveling tumultuously in all directions; the absence of a supervisor or indeed any possibility of supervision" (Wheeler 1954: 80). Wheeler, through his practice of laying out the trench, was instrumental in controlling this "chaos" by confining the archaeological site inside the

controllable formation of the Cartesian grid, and turning it into a neat enterprise. Contrasting the earlier photograph with a photograph of the Arikamedu excavation, Wheeler stresses the importance of excavation as a disciplinary aesthetic: "it shows a site neatly parceled out in readily controllable areas...every man knows what he is doing, and records are almost inevitably clear and sensible, the considered product of several pairs of critical eyes" (Wheeler 1954: 80). Analogous to the cartographical map that represented the spatiality captured within Cartesian grids, the Wheeler Method rationalized the landscape into a cartographic area fit for scientific intervention for it allowed complete control on the nature of data excavated, as we will see in the next chapter.

The ideological affect of the Wheeler Method was located in the Cartesian perspectivalism (Jay 1994) that encompassed a mass of land and transformed into an epistemic landscape. It virtually extracted the Cartesian grid from the confines of cartographical representation – the signifier - and imposed it on the actual physical landscape – the signified - for the extraction of data in a scientific manner. The Cartesian grid on the map was a conceptual survey tool to plot the site and it was neatly transferred from the map into the landscape. The disciplinarian apparatus employed to map the landscape ended up being inscribed on the landscape itself. The scientific practice of the inscription of the Cartesian spatial regime provided Wheeler with an epistemological foundation based on which the field was rendered legible, and which embodied the military disciplinarian regime that Wheeler introduced in Indian archaeology. The symbolic valence of the Wheeler method lay not only in its transformation of the site into a scientific zone that facilitated the performance of archaeology as a scientific practice, but also in its commanding visual presence as a spatial formation with scientific resonance. The employment of the Wheelerian grid allowed the archaeological landscape to be re-organized as a materialized conceptual schematic framework, through which the archaeologist-as-bureaucrat domesticated the physical world and organized it for his/her material practices.

Conclusion

The engagement with the excavation site as an archaeological entity, by subsuming and scientifically normalizing it through the cartographical schema of the survey-work rationalized the landscape, and made it epistemologically potent. This process was merely a way to domesticate the wilderness of the Harappan landscape and make it amenable to the ideological gaze of the scientific enterprise, by rendering the physical universe legible and coherent. But it

was with the imposition of the Wheeleerian grid on the landscape itself, that the first task of the archaeological project was executed. Now, the landscape, fashioned as a materialized epistemological spatiality, was "ready" for an archaeological excavation. The knowledge thus fashioned embodied by artifacts and ceramics that were removed, and the structures exposed, could be adequately controlled and ordered. The conceptual apparatus was now in place to produce and reproduce the visual and the disciplinary *affect* of the scientific excavation. With the emblematic ASI archaeological site in place, the task of unearthing and excavation could begin. The ontological and the epistemological structure of the Cartesian grid, of the Wheeler Method, now defined the lived experience of the ASI archaeologists, staff, students and the labor working at the excavation site. Their every day practices were physically structured by the imposing Wheelerian grid. Although the Cartesian framework of the Wheelerian grid was developed in order to systematize and organize the knowledge that was produced at a site, the next chapter shows how it was also instrumental in disciplining the ontological experiences of the people working at the site.

Chapter 5
The Practice of Discovery

Introduction

The ASI archaeological excavation was a carefully coordinated series of activities conducted by multiple actors over the course of many years. The journey from the discovery of the site, its survey, its excavation, to the final publication of the excavation report, took numerous years and thousands of man-hours from laborers, technical staff, archaeologists, officers, and site directors. This situation was characteristic of any bureaucratic, professional, and scientific activity anywhere in the world. The excavation lasted many seasons, three to four seasons being the average. But in extraordinary cases such as Dholavira, a large archaeological site, the excavation had lasted for twelve seasons between 1991-2005, with some breaks. During these years, more than a thousand trenches were excavated. By the time I did my ethnography, the monumental citadel with its fortification, lower and middle towns with their streets and houses, factory work sites, reservoirs and drainages systems, had been completely excavated and exposed.

The interaction between the various actors at the excavation sites from the Site Director to the daily-wage laborer was a well established and a regimented process that had been put into place by more than a hundred years of the ASI's cumulative experience as an organization. Since Wheeler, archaeological practice in the ASI had become regimented and consisted of carefully coordinated sets of processes and norms. The role of each worker in the archaeological excavation was well defined and distinct, and faithfully followed what had been specified by Wheeler in his manual, Archaeology from the Earth. Wheeler's influence was profoundly visible in the day-to-day working of the site, not just in the process and practice through which knowledge was produced, but also in the process through which the excavation site was managed and organized. I have shown in Chapter 4 that the epistemological framework of the Wheelerian grid was employed to produce an epistemic spatiality. In this chapter, I will concentrate on how the minute practice of archaeological knowledge production was also dictated and driven by Wheeler's method. Wheeler and his methods, as described in the earlier chapter, had a strong influence on the way the practice of archaeology unfolded in the daily goings-on at the site.

This chapter is theoretically influenced and framed by what Bruno Latour calls "science in

action." Here, unlike Latour's earlier laboratory work-space (Latour 1987; 1986) but analogous to his investigation of the processes of the scientists in the Amazon forest (Latour 1999), I localize my ethnographic intervention to the confines of the trench. I show how within the socio-political and the epistemological spatiality of the excavation site, archaeological artifacts are discovered and transformed into archaeological knowledge. I demonstrate that this process occurs, not in the larger context of the archaeological site, but in the micro-context of the trench. It involves all the various actors in the archaeology site, from the unskilled laborers to the technical staff, to officers at all levels in the ASI. My argument in this chapter is that the systematic archaeological process that produces scientific evidence for the construction of the narrative about past is itself a convoluted practice. It involves multiple levels of epistemological assumptions that are mediated by deep cultural and sociological frameworks of the actors working within trenches of the ASI excavations. These are cultural practices of constructing objective knowledge, which are based on numerous notions fraught with ideological and epistemological biases. In this chapter, I attempt to show through ethnographic descriptions how these epistemological ideological notions mediated by the Focauldian power/knowledge nexus function in the confines of the archaeological trench, which is the location for the production of objective archaeological evidence. This chapter will detail the daily practice of ASI archaeology in the excavation site focusing on the process through which artifacts and structures are recognized, discovered, recorded, and measured at the site. I will focus on the practice and process of discovery in the confines of the trench. Unlike the earlier chapter, which attempted to show how the ASI conceived and construed an excavation site, this chapter is concerned with the micro-practice of archaeology as a scientific practice in the context of production of archaeological evidence in the trench. However, my arguments about micro-practice in this chapter are framed by the larger socio-political makeup of the ASI as a postcolonial statist organization described in the earlier chapter. In the ASI excavation trench knowledge production as a scientific act was not carried out in isolation but was a product of the larger ideological, epistemological, and socio-political intersections that made possible the existence of the excavation trench.

The ASI excavation site was a "well-oiled" [*chalu aur chust*] bureaucratic and professional machine at work—conducting an archaeological excavation with "as much scientific care as possible for a governmental organization," an AA at Dholavira dryly explained to me during one of our conversations. Every morning at the stroke of the bell, the laborers who had arrived

earlier for the gathering roll call, would slowly enter the excavation site. I would sometimes get up very early in the morning to see this ritual as hundreds of men and women wrapped from head-to-toe in faded woolen blankets and shawls, emerged from the early morning mist. These workers would walk towards the excavation site in a regimental fashion, mostly barefoot, some in rubber slippers and others in worn-out plastic footwear, not with the military trot that Wheeler would have desired, but more like a rag-a-tag army of tired subaltern rebels. Carrying the tools of their trade — men hauling pickaxes and spades on their shoulders, and the women bearing a basketful of brushes, trowels, ropes and pegs on their head, they would enter their assigned trenches. Around the same time, from the tents of the camp, the ASI supervisors, students, and the Asst. Archaeologists along with the Site Director, would troop into the site begin the day's work – attired very differently in jeans, cotton shirts, synthetic wind-cheaters, leather jackets, sweaters, baseball caps, and hiking shoes. They would be carrying backpacks and bags stuffed with the tools of their trade –towels, water bottles, rulers, pens, pencils, clip boards, two types of measuring tapes, torchlights, brushes, a geometry box and a knife. In a matter of minutes, the excavation trenches would teem with activity that seemed organized and coordinated –with each worker performing their pre-assigned duty.

During my ethnography, I spent a big chunk of my time observing how a monumental excavation was coordinated and conducted to expose a small town or village over the course of a few months. In the process, hundreds of people were employed to remove thousands of tons of earth, revealing the monumental architecture of a Harappan habitation. Each actor at the excavation site had a specific task, which was carefully carried out in a synchronized manner. Looking from the top of the fortification mound at Dholavira, or the top of the habitation mound at Baror and Bhirrana, I could not fail to realize the labor-intensiveness of an ASI archaeological excavation. It was very different from archaeological practice in the contemporary western world and uncannily similar to descriptions of archaeological excavation in the colony. The men could be seen with pickaxes, digging the trenches and removing the earth into iron containers for the women, who would walk a few meters and throw the dirt in a dump. Some women could be seen sifting the excavated earth for artifacts. Others could be seen on their haunches with a brush and trowel, going through the earth in the trenches searching for artifacts. The Trench Supervisors, with baseball caps and cricket hats, clutching notebooks in their hands, could be seen sitting and jotting down notes. The technical staff with their helpers would be seen measuring the excavated structures and the ASI

draughtsmen drawing diagrams of these under the shade of a colorful umbrella. In the evening, under the slanting light of the setting sun, the photographers and their retinue of helpers would be seen taking photographs of the artifacts discovered onsite, or the structures or the floors exposed over the course of the day's excavation. For an outsider like me, the initial impact of such an activity, which reflected the organized might of a statist organization at work, was awe-inspiring. However, further into my ethnography, the façade of this coordinated action slowly gave way to reveal a complex ideological and epistemological process through which archaeological evidence was discovered and then constructed into knowledge.

Trench & the Quadrant

In Chapter 4, I demonstrated how the ASI archaeologists imagined and produced the epistemic spatiality of the excavation site. By employing the conceptual apparatus of the Wheelerian grid as a fundamental organizing principle, ASI archaeologists transformed a site with archaeological potential into an epistemological landscape that was ready for horizontal excavation. This chapter is concerned with the four-walled confines of the archaeological trench, describing how archaeological evidence is imagined, revealed, and exposed. The trench shall be center of my ethnographic focus, for the trench is a microcosm of the larger processes and the practices in the archaeological site. The daily practice of discovery, recordings, and excavations in the trench had a direct impact of the way the archaeologists framed the whole excavation site as it evolved and materialized into an epistemic archaeological spatiality. Thus, in order to comprehend how the ASI archaeologists conceived of an excavation and carried it out at the level of the whole site, it is crucial to shift our focus to the workings in the trench – both epistemological and sociological.

For the ASI archaeologist, the excavation site was conceived as a conceptual macro-entity, whereas the trench was "where real archaeology happens." The Co-Director of the Dholavira site explained, "the site is something you write reports about. Real archaeology is done in the trench. It is in the trench that you connect with ancient civilization. The site is about the big picture; the trench is where you do archaeology. This is where you dirty your hands." We were crouching on the floor of a trench in the citadel, with brushes in our hands, clearing up a little patch of earth where a small scatter of carnelian beads had been found. We were on the floor of a mature Harappan room, surrounded on three sides by stone-cut bricks, parts of

which were concealed in the balk that cut across the room. There was a hearth tucked in one corner of the northeastern wall of the room that had been excavated a few days ago. Half an hour before, I had been sitting with the Co-Director in his mud and thatch hut [*boonga*] chatting about the state of cricket in India (Pakistan was going to tour India in a month's time), when a young laborer, gasping for breath, had barged into our room and informed that a necklace had been discovered in the citadel. We rushed to the trench. On reaching, the Co-Director immediately pulled out the knife from his trousers' back pocket and started scraping the earth where a few "typical Harappan" carnelian beads were discovered. After a few gentle knife jabs and scraping, he told the Trench Supervisors: "Good job! I knew this was a very rich trench. You will find more antiquity. Dig carefully. Take the measurements properly and expose this carefully. Don't dig any more, just clean up. Prepare the subject for photography. I want an in situ picture. This is beautiful evidence."

Soon after, we sat on top of the balk of the half a meter deep trench. The Co-Director had decided to step back. He was observing as the Trench Supervisor, who was a student, got on her knees and took over the task of exposing the carnelian necklace with a small paint brush and surgical instruments (part of a high school level dissection box) to expose the scattered carnelian beads on the floor of the trench. The laborers meanwhile were instructed by her to clear the floor of dirt and "prepare the subject for photography." As we were observing, the Co-Director explained, "the trench is the whole-n-soul of archaeology. If you cannot dig the trench properly then the whole site will get messy [*agar trench kharab tarike se khoda toh poore site ka satyanaash ho jaye ga*]." He often interrupted his conversation with me with instructions to the laborers crouching with brushes and working in the trench to do their work properly. He continued: "I train students how to dig a trench correctly. A good excavator is one who can control a trench systematically. Harappan sites are big. You cannot dig the site all at once. You have to do it bit by bit. And that's why we need balks and trenches. How much ever you want to criticize Wheeler - this is the best method for Indian archaeology. It is fashionable for you young people to criticize Wheeler, but whatever said and done, his methods are the best for us. Like him, I also try to teach my students to do perfect archaeology."

The spatiality of the excavation site was fragmented with the micro-spaces of trenches as manageable epistemological units that could order the knowledge produced at the site and

controlled the people working at the site. For the ASI archaeologist, the trench was the functional unit of archaeological excavation at a daily level. It allowed for efficient control and management of both the knowledge produced and the personnel working in it. Sociologically, at the micro level, the daily activity of the ASI archaeologists, supervisors, students, and the laborers were all situated in the trench. It formed the vital fulcrum of archaeological activity at the site; both in the way that the materiality of knowledge was produced, and also in the way it was administered and controlled. The organizational role of the trench was that it ensured control of the excavation process while its epistemological functionality lay in providing discipline to the knowledge production mechanism.

My informants regularly emphasized to me the importance of the trench located within the Wheelerian grid. Most of my informants knew that I was trained at Deccan College, which as an institution, had rejected the necessity of the Wheelerian grid in Harappan and Chalcolithic sites. Once, during a conversation with a young AA who had just graduated from the ASI's Institute of Archaeology in Delhi, I was told: "How can you excavate without trenches and balks? It is easy to say that excavation should follow the logic of the structure. But you will get lost. It is okay to do away with balks in small sites. But look at Dholavira." Such justification was very common and almost all my informants firmly believed that it was not possible to dig a site without trenches in a grid: "You need trenches and *balks*. Or you will just dig for eternity and get confused. Trenches within balks are essential. They are the most logical way to dig. You have to dig step-by-step [*ek-ek kar ke*] — trench-by-trench. It is impossible to just dig anywhere you find structure. Destroy the balks later on when you need to study the site or a structure as a whole. But you need the balks when you are just starting to dig. Or you will go crazy [*aap pagal ho jayenge*]," retorted the AA when I asked him: "Why balks? Why not do away with balks since they can become a hindrance?" Within the imagination of the ASI archaeologists, the trench had an epistemological valence similar to the logic of the Wheelerian grid, which was given without an alternative: "It is the only way to dig. There is no alternative [*koi alternative nahi hai*]" explained the AA at Dholavira.

I observed that the ASI archaeologists, who worked daily in the site, rarely engaged with the site as a whole. The excavation as a whole was always conceptualized as an abstract entity; at the level of archaeological practice, archaeologists engaged only with a quadrant, a trench, or a few trenches at a time. "The whole picture is always missing," complained an AA. "Day in

and day out, all you do is just focus on one quadrant or one trench. That is the discipline of research work but it can be boring at the same time." I was sitting with the AA seven meters below the surface, inside a deep vertical trench in Hansi that had been excavated to ascertain the stratigraphical history of the site. Since Hansi was a monumental mound, the Site Director had decided to first excavate a few trial trenches before he started with horizontal excavation. The AA was frustrated because for the past one-and-a-half months, they had been digging with the hope of "hitting" the Harppan layers, but even after seven meters of digging, they had just reached Painted Grey Ware (PGW) culture (first millennium BCE). "I know that only through working in a trench will you be able to create knowledge, but one feels a thirst to dig up the site whole at once. But that would be unscientific. Complete chaos. You have to admit, Wheeler was right about the balks and trenches. It just makes things so easy." In daily practice, the Wheeler Method was useful because it was systematic and more importantly, it provided a systematized way to excavate the rather unwieldy Harappan site. In order to comprehend workings at the micro-level of the trench, it is important to be conscious of its umbilical linkage with the Wheelerian grid and the larger sociological and epistemological universe that I have described in the chapters before.

The trench was the logical outcome of the Wheelerian grid and represented the conceptual continuance of the modernistic and scientific ideology embodied in the Wheelerian grid. It was a rational product of the conceptual framework in which the ASI archaeologists imagined and carried out archaeological excavation. Its epistemological ramifications were underscored by the fact that it was a precisely bounded physical location of archaeological activity at a site, where archaeological evidence was excavated, recognized, discovered, and recorded. As Wheeler convincingly notes of his method: "Be it repeated that a great merit of the 'square' method is that it localizes both control and record" (Wheeler 1954: 85). It was in the confines of this 'local' space that the micro-practice of archaeology as an epistemic act was bounded. The trench was the four-walled laboratory space, which exemplified archaeology as a scientific process. This is the epistemic zone where archaeological evidence in the form of artifacts, material culture, ecofacts are recognized, discovered, recorded, and transformed into evidence.

Wheeler referred to the trench as the "square." For Wheeler, the term square was more appropriate than the trench, as he wanted to simultaneously emphasize the geometric and

therefore the scientific nature of the grid, and also distance his approach of excavation from the earlier and much maligned open area excavation. On Wheeler's view, trenches, as dug before he re-conceptualized them as squares within the structured confines of the grid, were:

...bad for more than one reason. They mess up the site unless very wide (when they are, in effect, cumbersome area-excavations). They are liable at any considerable depth to become excessively confined and difficult to work in, their scarification cannot be viewed compressively and at adequate range, and above all, lateral enlargement complicates the record to an extent, which endangers accuracy (Wheeler 1954: 81).

For Wheeler, the trench represented an unscientific method, an "old practice" of excavating an archaeological site. It reflected a process of excavating the earth that was determined by probability rather an instrument of scientific intervention: " ...it was to a large extent 'shooting into the brown' on the off-chance of bringing down a bird" (Wheeler 1954: 81). The term "square" invokes a mathematical and exact universe that diminishes the "mess" that is symbolized in the term "trench." The use of the terminology of the square instead of the trench is rhetorical, and alludes to the scientificity and the exactness of the Wheelerian grid. However in the daily practice of the ASI, the term square was not used at all. The term trench continues to be utilized. It was one instance of pre-Wheelerian practice that the methodological advances of Wheeler were unable to dislodge, but the scientific and the geometric features of the Wheelerian Square were synonymous with the contemporary trench.

Notations in the ASI site notebook continued to use the term "square," bracketed in Hindi as *varg*—meaning section, division, or separation. Both terms symbolized the productive possibility of the Wheelerian grid, which was its ability to fragment spatiality and to domesticate it within its modernistic grasp. The productive characteristic of the Wheelerian grid was its ability to transform a large landscape into controllable and manageable units. The managerial quality of the Wheelerian grid was that it allowed any space to be further subdivided and fragmented for efficient control over the production of evidence at the excavation site. Its conceptual configuration allowed the archaeologists to subdivide a given archaeological spatiality into smaller units of trenches and further fragment each trench into quadrants. The ASI trench was a ten-meter by ten-meter square divided into four equal quadrants, separated by a half-meter balk. This further fragmentation of the trench was a dynamic attribute of the gridded apparatus, which allowed for theoretically unlimited fragmentation of the earth to enhance manageability and control of the material culture

produced in the site. A quadrant was the product of further fragmentation of the archaeological site, with the aim of managing and organizing archaeological evidence.

The smallest unit of such fragmentation that I observed in an ASI site was the one-meter by one-meter control pit which was usually excavated before a "dig." The control pit was an investigative micro-trench that was dug in order to gain information about material culture that was situated below the whole quadrant or the trench. Wheeler describes such a pit as "a small cutting, about two and half feet square, split by the supervisor himself or by a trained man under his eye, to a depth of one and half to two feet lower than the average level of the work" (Wheeler 1954:84). Such an investigative square in a trench was necessary to "enable the supervisor, with a minimum disturbance of the strata, to anticipate the nature and the probable vertical extent of the layers which are being cleared by his main gang. It is a glimpse into the future of his stratigraphical work" (Wheeler 1954:84). The theoretical impetus of the control pit was similar to the quadrant, trench, and the grid - its emergence in the archaeological site was rooted in the archaeologist's desire to control the process of excavation. As I have often tried to show in this dissertation, the primary impetus of an archaeological excavation was to optimize and control both the epistemic spatiality that had archaeological potentiality and the process through which archaeological evidence was produced. The trench and then the quadrant is an outcome of such an epistemological approach towards knowledge production and control of people.

Thus before any trench was dug, the ASI practice was to fragment the quadrant into four control pits. The excavation would begin with digging in one of the control pits to test the nature of the deposit. It was only after this that the rest of the quadrant was excavated. Once a quadrant had been excavated to about a meter in depth, the whole trench would be excavated. This process was very strictly followed at Birdana, where the Nagpur Ex. Br. was conducting their excavation. The AA, who had been awaiting promotion for the past ten years, explained: "Nagpur Ex. Br. is the oldest excavation branch in the ASI. Although I have been transferred here just a couple of years ago, I have learnt a lot about how to do excavation the proper way. I have spent most of my career either in the Circle Office or in the Museum, but this is the first time I am working in an Ex. Br. Here the quadrant is taken as the most important piece of the excavation. First, study each quadrant thoroughly and then divide the quadrant into four equal control-pits, and then dig one at a time. That is how you control the digging. This is the

method that comes from Wheeler. Seniors who worked with Wheeler personally trained all the technicians here. This is the best it gets in the ASI."

The trench and the quadrant served as the primary areas of jurisdiction for the archaeological excavation. All the excavation work in the ASI site was formulated and regimented by the physical and the bounded spatiality of the trench. The disciplining ability of the trench emerged from its ability to control the three-dimensional excavated space that was surrounded by the trench walls. Before the excavation, the trench was primarily an inscribed space on the landscape, which was demarcated by the grid lines that encapsulated it. As the digging began, the two-dimensional inscribed space gained its third dimension, and the inscribed space became deeper as the trench became a three-dimensional epistemological and social spatiality. In the process, the four walls of the trench begin to emerge and serve as the disciplining frame within which archaeological evidence was fixated. It was this four-walled, three-dimensional, spatiality embodied by the trench that made the archaeological excavation a scientific act. The possibility of control and discipline that was epitomized by the trench, within the larger spatial disciplinarity of the grid, imbued the mere act of digging for material culture hidden in the earth with a sense of rationale and focus. The Wheelerian grid that encompassed the archaeological landscape and each trench that formed a unit of the grid worked in conjunction to turn the excavation site into an epistemic location. If the Wheelerian grid transformed the archaeological landscape into an epistemic landscape, then the trench converted the excavated earth into an epistemic unit. By subdividing the archaeological landscape into a series of trenches that were lined in a grid formation, the landscape was fragmented into manageable units. Each trench then became its own encapsulated universe, with its own makers of control and organization of the excavated material. The disciplinarity of the Wheelerian grid that managed the excavation of the whole site worked in tandem with the controlled spatiality of the trench.

Similar to the grid, the impact of the trench went beyond its epistemological significance to affect the daily lives of the workers at the site. The trench not only controlled and disciplined epistemological production but also managed and controlled the workers at the site. ASI archaeologists employed the trench to subdivide the labor force. A trench supervisor headed each trench; this position was often assumed by student archaeologists. At sites where student supervisors were not available or where the ASI archaeologists had opened too many trenches

because they had decided to expand horizontally, several trenches would be supervised by a single supervisor who was an AA or a former student. Under the trench supervisor was a "gang of laborers," which included a trench leader who was always male. Though there have been women trench leaders from time-to-time, I rarely saw any females in positions of leadership during my time at the sites. The key skill of the trench leader was that he or she was literate and was expert enough to recognize the artifacts or features found in the trench by their semi-technical nomenclature and inform the Trench Supervisor about discoveries in the trench. Under the trench leader were usually five or six laborers who worked in the trench. The daily life of these groups of people (other than the Trench Supervisor who was not a student) were bounded and constrained by the trench. Each laborer had to take permission of the trench leader to get out of the quadrant –to drink water, to go to use the make-shift toilets or to get beyond the confines of the trench for any another purpose. The Wheelerian grid forced an ontological discipline that structured the daily lives of the people who were involved at the site, working for more than eights hours a day, where the trench controlled their physical movements.

The physical characteristics of the grid complemented the organizational hierarchy of the ASI excavation team. In an extraordinary way, the hierarchy of the postcolonial organization slipped comfortably into the disciplinary and controlling structure of the Wheelerian grid. The grid and the organizational hierarchy at the site each contributed to the other's disciplinary technique—making the excavation site both an organizationally and an ontologically oppressive space of knowledge production. Lowest in the hierarchy were the laborers who were most powerfully controlled by the disciplinarian universe of the Wheelerian grid and its confined trenches. Every morning after the roll call, each laborer had to present him/herself at the trench where he or she was working along with the other members of the trench and then spends all the day working inside it. This daily practice was lodged between modernity's two disciplining apparati at the excavation site: the factory time and the gridded spatiality. The encapsulated space of the trench had a profound impact on the way the laborers carried out their work in the trench. The moment they left the trench, the trench leader of the Trench Supervisor marked them.

Before I proceed, I will take a historiographical detour that will take us to Wheeler and the period of monumental discoveries between 1944-48. The next section of this chapter will

elaborate on the practice and usage of stratigraphy, which forms an essential epistemological tool in any archaeological excavation. In the ASI trenches, however, its practice was ossified to the way in which Wheeler both formulates and performs it's existence. Although the ASI archaeologists before Wheeler had established chronological relationships between material culture and cultural layers, this process had rudimentary value. Wheeler placed stratigraphy at the crux of his archaeological practice, along with meticulous recording, and temporal narratives of the site. These three principles frame the epistemological significance of the Wheeler Method in the trench, and it is this theory that dictated the micro-practice of archaeology in the grimy trenches of the Saraswati Heritage Project.

Mortimer Wheeler and Stratigraphy as temporal cartography

The appearance of stratigraphy in Indian archaeology as an essential tool to determine the temporality of the cultural layers occurs with the advent of Wheeler. Prior to Wheeler, like most parts of the world where cultural-history archaeology was in vogue, artifact typologies and classifications determined the chronology of the site, along with stratigraphy. Although the idea of stratigraphy was known in Indian archaeology by the time Wheeler came, it was he who provided an objectivistic and scientific agency to it. For Wheeler, it is only through planned and disciplined excavations, with an emphasis on three-dimensional recording, that a proper stratigraphic sequence of the site can be created. Such a sequence functions more like a cartographic exercise with the capability to map time. By the time Wheeler came to India, artifact typologies and classification systems were a standardized means through which relative dating of an excavation site and its cultural layers was done. In this process, the principles of stratigraphy were rudimentarily implemented, playing a secondary role in the dating of the chronology of the cultural layer in the trenches. It is with Wheeler that stratigraphy and its emphasis on absolute chronology enters into Indian archaeology (Chakrabarti 1988; Paddayya 1995): "...absolute chronology is essential alike to the appreciation of varying tempo of human achievement, and, above all, to the establishment of the cultural inter-relationships which help to rationalize human 'progress'" (Wheeler 1954:56). It is this concern with absolute chronology that frames Wheeler's archaeological intervention in India and it is here that he perfected the method of stratigraphy. For Wheeler, absolute chronology is important and at the crux of archaeological work:

It may seem reactionary and perverse to reaffirm, as I do, at the beginning of a book [Archaeology from the Earth] on archaeology in the field that mere dates are still of primary

and ultimate and unrelenting importance. And by dates I mean not simply those nebulous phase and sequences, those dates-substitutes, with which archaeologists often enough try to bluff us. I mean time in hard figures. I mean Bradshaw...It is important but not enough to know that in twentieth century A.D. an aeroplane flew from London to Singapore. It is almost equally important, in our estimate of human achievement, to know that in 1950 the aeroplane took 50 hours for the journey, and in 1999 only 50 minutes. Do not let us forget the significance of tempo; and that implies a time-table in the most literal sense, nothing less (Wheeler 1954: 38-39)

It was not surprising that Wheeler framed his research program in India with a chronological focus. This focus has had a direct influence on the way he organized archaeological excavations in India and the practice through which stratification of layers in the excavation trench became the site for mapping time. In the absence of absolute dating techniques like carbon 14 and others, Wheeler argues that proper stratigraphical recoding is more absolute than the relative dating obtained by studying artifact typologies and classification (Wheeler 1927).

Academically, there are two central objectives to Wheeler's intervention when he arrived in India in 1944. The first goal was to rectify the ills of the ASI that were reported by Sir Leonard Woolley in his report on the state of Indian archaeology in 1939 (Woolley 1993 [1939]). This goal, as I demonstrated in an earlier chapter, had a disciplinarian impetus and was largely aimed at restructuring the institutional apparatus of the ASI. The second objective of Wheeler's intervention was intellectual and had a chronological focus. Wheeler aimed to solve the temporal dysfunction of the Indian past, both in the Northern plains where the Indo-Gangetic civilization thrived and in the Southern plateau and the coast where multiple kingdoms rose and fell. In both cases, Wheeler's intellectual intention was driven by a desire to uncover the chronological absence that marked the archaeology of these regions. The problems in the North, according to Wheeler, were much simpler and involved in uncovering a temporal hiatus between the Harappan Civilization and the Northwestern kingdoms of the sixth century BCE. The dates of both the periods were archaeologically available through comparative and analogical dating with the Western world- Indus civilization to the Mesopotamian and the later settlements with the Achaemenid Empire. For the South, the problem according to Wheeler was far greater, because unlike the North, the settlements of South India did not have any comparative, temporal, or analogical relationship with the

western world (Wheeler 1955:188). As Wheeler puts it:

> In the south of India the archaeological problem is, in a sense, vaster still. There we have no dated contact with ancient Mesopotamia, no intrusive Persian Empire...[F]or earlier periods, material is abundant, its inter-relationship unknown. It is a jumble of words with no consecutive meaning. But here again, planned work can gradually bring order and significance into chaos (Wheeler 1955: 188).

Correlating Indian sites with Greco-Roman artifacts became one of the most important agendas for Wheeler. He notes in one of his texts that in order to deal with what he describes as, "the 'Dark Ages' of the Vedic period," the first requirement is: "...to determine its delimiting phases with all possible exactitude" (Wheeler 1949: 5). In effect, the scholarly focus of all the excavations that Wheeler undertook in India (Taxila, Arikemedu, Brahmagiri and Harappa) during his four-year tenure as the Director General of ASI, was on solving this dysfunctional temporality of Indian archaeology.

It was in this context of Wheeler's work in India that his idea of stratigraphy becomes profoundly important. It is through excavations that were marked by methodological emphasis on stratigraphical chronology that Wheeler is able to provide South India with 'concrete' dates. He undertook two excavations in Southern India – the Roman trading station in Arikemedu and the megalithic site of Brahmagiri. In both cases, Wheeler employed stratigraphy to fix the chronology of the sites. The site of Arikemedu provides an extraordinary example of how colonial archaeology under Wheeler worked, for the intellectual thrust of ASI could only conceptualize an archaeological site in India within a chronological framework of a European temporality. This notion of the European time in archaeology has been largely defined and constructed by a Western fixation with classical archaeology and antiquity in oriental scholarship. In India, from the moment of the discovery of its past, colonial scholars and antiquarians have attempted to fit India within the confines of European cartographical and chronological imagination (Raman 2002). In its incipient stages, the ASI was also driven by a dual necessity of cartographically mapping India and chronologically dating Indian monuments and sites through its contact with the West. Alexander Cunningham conducted an archaeological survey of most of India, relying on the travel accounts of the 5th-9th century CE Chinese travelers. He located the contemporary provenance of ancient Buddhist sites by comparing them with Chinese accounts. Cunningham then proceeded to strip the Stupa and drill holes in the bottom of the sites to locate Buddhist relics and caskets. As he correctly

assumed, he found Indo-Greek coins, which were useful in dating other discoveries where the date was in doubt. For Wheeler, and most of his earlier Orientalist predecessors, ancient India lacked historical imagination in the linear European sense. Thus, it was possible to create a chronology of ancient India only through its contact with the West. In the process, the Indian past was subsumed within the temporal dimensions of European time. By the early twentieth century, scientific archaeology had laid its roots in India, and the earlier antiquarian impetus of the ASI had given way to a focus on excavation of ancient Indian sites in order to create a chronological sequence.

It is in the South that Wheeler's fetish for fixating the chronology of the Indian past brings into clear focus the colonial subtext of his scholarly enterprise and the ideological subtext of stratigraphy as Wheeler used it. Like in the North, where the chronology of Indian archaeology was fixed by correlating the artifacts and sites on the basis of the known contact with the West, Wheeler desired to establish a similar datum-line for the South. This was to be provided by the Roman trade contacts in coastal India. His search took him to the coastal site of Arikamedu in the French colony of Pondicherry. Along with the known horde of Roman coins, Wheeler discovered Roman ceramics. Sharing his discovery in a note to M.V. Taylor, President of the Society of Antiquaries of London, Wheeler's excitement is noticeable:

> Today you will be faintly entertained to hear that we have found the first Arrentine pottery stamp known, so far as I am aware, from India [Pause for drums and trumpets]. The place being Arikamedu or Virapatnam, two miles south of Pondicherry. But seriously, it is slightly romantic to find out here under the coconut palms the identical stuff that you or I are used to in other climes. The beauty of it is of course, that we are getting hitherto wholly undated Indian culture in association with it, together with substantial brick buildings which are just beginning to make their appearances' (AACD, File No. 19/14/44; 1944. Dated 7th April 1945).

Explaining the importance of the find to Lt. Col. Stuart Piggot, British Air Commander of Southeast Asia and New Delhi, Wheeler notes: "The place [Arikamedu] clearly contained a Roman colony in the first half of the 1st century A.D., in connection with the semi-precious stone trade. The beauty of it, of course, is that here we shall get at last our synchronism with native Indian stuff, and a firm chronological datum line for South East India within a margin of 50 years. As I said before, don't laugh' (AACD, File No. 19/14/44; 1944 D.O. No. 517/C. Dated 9th July, 1944).

Wheeler conceptualized the significance of Arikamedu to the chronology of South India within a cryptographic metaphor. For him the site was a "bilingual" one where, "the unknown local culture is dated from the known foreign culture, just as Egyptian hieroglyphs were partly deciphered from the parallel Greek version on Rosetta stone, or Kharoshtri from the bilingual inscriptions on the Indo-Bactrian coins" (Wheeler 1946: 1). The subtext of the cryptographic metaphor is situated within the colonial ideology of taming the unknown. This process of framing and structuring the Indian archaeological past within the metanarrative of the western civilization can been seen in the subtext of a seemingly innocuous remark that Wheeler makes about the Arikemdu excavations: "The erratic cuttings of our French predecessors on the scene were methodically superseded and extended by school-trained grids and graduated stratigraphy in the busy hands of students already trained to anticipate this sort of things – the emergence of familiar western products in the meaningful association with the still unknown and variable output of the east" (Wheeler 1976). Thus, Wheeler does not conceptualize Arikamedu as an archaeological site in its own indigenous terms or within a southern Indian temporality, but it has to be historically situated within the metanarrative of Western history. In this hegemonic imagination, Arikamedu is relegated to the fringe of classical historiography, and at best named as a trading port beyond the frontiers of the Empire. By negating its indigenous temporality, Arikamedu's past is seen as playing a peripheral role in the larger expansionist progression of the classical world. Benign trade and commerce were typical in Arikamedu, unlike those cases in North India, which were largely framed within the invasion narratives of the west.

In Wheeler's stratigraphic imagination, Roman Arrentine ware and amphorae from the Mediterranean were central to delineating the chronology of Arikamedu. These two forms of material cultures were utilized to provide a datum line not just to the site but also to the whole of South India, "...upon the imported Mediterranean wares the whole chronology of the site, and its special importance therefore to Indian archaeology depend" (Wheeler 1946c). Wheeler uses these two ceramic types as a control mechanism—to discipline the atemporal artifacts of the native site:

> ...subsequent to our date A.D. 50 there were, in the Southern Sector, several successive stages of construction and reconstruction, accompanied by some modification of the associated Indian pottery. These developments were controlled by

> two unifying factors: a general continuity in the main units of the plan, and the occurrence in all strata of shreds of Mediterranean amphora" (Wheeler 1946c: 24).

Thus, stratigraphy under Wheeler transforms into an ideological practice of the past, which is framed typically as a scientific practice but is mediated through imposing narratives of the Western classical antiquity. This can be more acutely seen, when on the basis of the stratigraphical occurrence of Roman material culture, Wheeler not only demarcates Arikamedu's functional character as a peripheral trading post of the classical world but transforms it into a colony of the Roman empire. He claims: "The historical indications are that the consolidation and development of Roman trade with the east was a product of the unification of the western world under Augustus (23 B.C.- A.D. 14)...therefore, the Roman occupation of this site is unlikely to antedate the principate of Augustus" (Wheeler 1946c: 22). The usage of stratigraphy in ASI archaeology emerged from such a Wheelerian genealogy, which forcefully correlates the material culture excavated with the stratigraphic context. Often during my conversation with ASI archaeologists working in the trenches, the importance of stratigraphy and its usage in Arikamedu was narrated.

For Wheeler, stratigraphy represented a practice through which the science of archaeology in the daily practice of excavation and discovery could be made robust and systematic. In the larger scheme of Wheelerian archaeology, stratigraphy enabled archaeologists at the site to have more control and discipline over the temporal axis of archaeological practice. The archaeological roles of the trench in Pre-Wheelerian archaeology in India was primarily to excavate artifacts and expose structural features, which were evidence for both the materiality and the temporality of the site. With Wheelerian archaeology, the trench became both an epistemological unit that contributed to unearthing of past material cultures, as well as the location to situate the temporality of the site through its stratigraphic section. Before Wheeler, however, the temporality of a site was primarily determined through post-excavational typological and classificatory studies of artifacts and material culture. Wheeler introduced another element into archaeology through which temporality of a site could be ascertained and established. By emphasizing stratigraphy, he does not undermine the importance of typological study of material culture but sharply shifts the focus on establishing the chronology of the site from post-excavational typological analysis to the moment of archaeological excavation. This shift had an instrumental impact on field archaeology as it weakened the role of the typological expert in the museum and made the archeologist in the

field the central figure in determining the chronological sequence of the site. By making stratigraphy as the scientific conduit through which absolute temporality of a site could be determined, Wheeler was successful in shifting the focus of doing archaeology from the museum into the field. This emphasis on the field had a tremendous impact on the practice of archaeology in India. Wheeler stresses the importance of doing scientific archaeology in the field during his work with the Taxila Field School. He forcefully placed the spatial and the temporal loci of archaeology in the field for Indian archaeologist – an emphasis that remained during the ASI field school.

Stratigraphy and the Section

During the post-lunch session one late afternoon in Dholavira, I was accompanying the Co-Director of the site as he went on his rounds, inspecting all the trenches. After several hours of touring trenches in the citadel, southern and eastern reservoirs, we came across a trench in the Middle Town. The Trench Supervisor was a second-year student of the Institute. The quadrant was several meters deep and was "hitting" the early Harappan layers after having dug through habitational deposits of the mature Harappan layers. According to the Co-Director, this was trench was crucial to the objective of the excavation. In this trench, it was to see: "what remained below the complex structures of the mature Harappan township." Upon entering the quadrant, the Co-Director suddenly flared up without any warning. In stern tones, he rebuked the Trench Supervisor: "You have destroyed my work. You cannot just mark lines on the section wherever you like. These lines have meaning. You should not mark lines on the section if you do not know their significance or if you cannot explain the meaning of the lines. There should not be any useless lines on the section [*faltu lines nahi honi chahiye*]." The Co-Director was upset because the Trench Supervisor had scratched multiple lines with his knife to demarcate the stratigraphy on the section. He continued, "You cannot have lines on the section run like tree-roots and tributaries of a river. They have to be bold and strong. Your trench is now confusing. It does not convey any meaning". He then proceeded towards the trench walls and knelt down. Removing his spectacles, fitted with shortsighted lenses, he carefully peered into the surface of the section for a closer inspection. After a few seconds, very reflexively, he pulled out his knife from the back pocket of his trouser and began scraping the lines made on the section and erased them. He then told the Trench Supervisor, who was sheepishly standing in one corner of the quadrant: "ask your section cutter to redo the section. I want it to be done by the end of the day. I will come and show you how to make

correct lines of the section tomorrow morning." As we came out of the quadrant to resume our site inspection, he remarked, "You know how important stratigraphy is in archaeology. It cannot be taken lightly. It is as important as finding antiquity. Even in trenches, where we do not find any antiquity or structures, if we get good stratigraphical section – it is very useful evidence. It is important to teach these students the importance of stratigraphy. Archaeology is not just about things, it also about time."

Early the following morning, we were back at the trench. Under the slanting light of the rising sun, the Co-Director examined the layers of the stratigraphy on the section. One by one, beginning from the top-most surface (i.e. the most recent layers chronologically), each layer was closely scanned and observed. With his knife, he checked each layer – jabbing the section surface to check its density and thickness, drawing out the soil from the section and feeling the texture in between his fingers, pressing his thumb on surface to check its compactness. He also sprayed water on the surface to see how the color changed. Soon, he scratched deep bold lines on the surface of section, where the layers of the strata separated. This process was repeated at each layer. After nearly half an hour of such close examinations and demarcations of each visible layer on the section surface, he commented: "Look, this is how stratigraphy has to be studied and made. You cannot have thin, unsure lines. They have to be thick and bold. Remember, you are the master of the trench. No one else on the site knows your trench better than you do. So when you are drawing stratigraphy lines you have to be confident of what you are doing. It is ok if you commit mistakes, we all do. But be sure of what you are doing. That's how you will learn."

In the gridded universe of the excavation site, it is the walls of the trenches that provide the stratigraphic evidence of the site. The walls of the balks that subdivide the archaeological site into trenches and quadrants are the epistemic materiality that the Wheelerian grid produces. These walls play an evidential role in delineating the stratigraphy of the site since the physical embodiment of the stratigraphy is the section—the vertical surface of the four walls (balks) of the trench that enclose it. The section is both a material and visual manifestation of the stratigraphy that reveals the layers of accumulation of earth on its surface. In the enclosed spatiality of the trench, it is the epistemological interplay between materiality and the visuality of the section, through which scientific archaeology of stratigraphy is articulated. For Wheelerian archaeology, the section was the key location where the science of stratigraphy

was practiced and performed: "the basis of scientific excavation is the accurately observed and adequately recorded section" (Wheeler 1954: 22). The wall surface is a material cross-section of the archaeological site, analogues to the section of a biological sample after dissection. The excavated trench is essentially a surgical fissure into the landscape not just to discover past material culture but also to expose the depositional content of the site on the section to situate its temporality. The surface of the trench walls materially reflects its chronology, as the section visually shows the various stages of cultural accumulation over time. The section is the visual and the material representation of temporality in the trench and it is by systematic study and recording of the section that the science of stratigraphy is carried out. The science of archaeology for ASI archaeologists in the trench therefore consisted not just in excavating the trench in a codified manner to unearth and expose past material culture, but also to explicitly reveal the stratigraphy on the section.

For ASI archaeology the process through which the sections were observed, read, and interpreted, was essential to the articulation of the science of stratigraphical archaeology. Wheeler had also emphasized that it was through the accurate practice of the observation of sections that the scientific practice of stratigraphy was performed:

Observation in different lights at different times of the day may help. In a difficult and important section, observation may be continued over a period of days before certainty is reached. And finally an attempt must be made to 'read' the section – discriminate, without prejudice, between the more significant strata and the less significant differentiation of strata...it is not enough to identify layers, although that is, of course, the essential step; it is the task of the archaeologists to interpret them, to understand the sentence and transliterate it [italics in the original](Wheeler 1954: 60)

This fragment by Wheeler from his chapter on stratigraphy in the Archaeology from the Earth, is an explicit statement about the epistemological essence of stratigraphy in the ASI excavation trench. The science of stratigraphy for archaeologists was about the systematic practice of observation and the accurate process of recording –the interpretitive moment was subsumed under these two practices. Through consensus building in the trench, which involved numerous actors with credibility, the interpretative analysis of the stratigraphy was turned into an objective category. Through the ontological act of observing the visuality of the section and the inscriptive act of scratching the surface of the section with a knife, the stratigraphical section was transformed from an interpretative epistemic artifact to an objective

category. But it was only after it was labeled that it signified the factual chronology of the trench.

The focus of "perfect observations" of the section was a central concern of the archaeologists in the trench. The other central concern was with the precise and the accurate recording of the artifacts found on the floor of the trench. A considerable amount of time was spent in the trench in the daily micro-practice of archaeology to enable reading, interpretation, and recording of the section. The emphasis on accurate observation, interpretation, and recording was an iterative practice of the daily science of archaeology that was dutifully followed by archaeologists as underscored in Wheeler's Manual:

> In practice, the identification and correlation of the strata or layers which represent the successive phases in the archaeological 'history' of a site is one of the principal tasks of the excavator and will occupy a major portion of his time...The task is one which involves clear and logical thinking reinforced by experience and infinite patience (Wheeler 1954: 60)

The science of archaeology in the trench was both a process through which not just systematic excavations of material culture were retrieved and recorded, but also the sections were prepared observed, interpreted, and recorded. In ASI trenches, the necessity to read and observe the section began soon after the first dig, which usually ranged between six to ten inches, where the habitation deposit was "dense," [*jyada*] and less depth if the habitation deposit was "thin" [t*hoda*] –informed an ASA, who had spent a considerable amount of time working on both Harappan and Historical sites. He explained that unlike Harappan sites, stratigraphic observation did not begin before a meter had been dug in Historic or Early Historic sites, "where the habitational deposit was very thick and deep." But at Harappan sites, especially early Harappan sites where the total habitational deposit of a site might not exceed more than a couple of meters and where mud brick architectural features were prolific and difficult to differentiate from the soil, "close stratigraphic control had to be maintained. Constant observation of the layers and deposit in the section is essential in digging a Harappan site," explained the ASA.

On site, I observed that the epistemological process of stratigraphy began with the interpretation of the soil layers in the trench by the Trench Supervisor who noted his readings in the trench notebook. Since s/he was responsible for the trench, s/he was encouraged by

senior archaeologists at the site to mark on the section surface the different layers with his knife. This process was done through close observation of the layers over the course of a few days. Numerous techniques were used to read the layers properly. Most often, the stratification on the section was taken early in the morning or late in evening when the sun was in the horizon. Another method was to spray the section surface with water to study the layers. Both these processes were essential, as an ex-student Trench Supervisor informed me: "Due to extreme heat, the soil moisture evaporates very fast. These two methods were not the only ways to observe the character of the layers." Reading stratigraphy was not focused on just visual observation but also included, "feeling the texture of the soil;" "touching the section with fingers;" "poking the section with knives." The layer in the trench was attributed with agency and the only way to comprehend it was to "understand its behavior." During a lecture in the trench, an AA instructed the Institute students that the most vital thing about the stratigraphic layer was to "observe its behavior. See where it goes. See how it moves. Feel its compactness. See how it changes color. Feel its texture. Taste it, if necessary. You have to remember that each layer is real. In order to understand it you need to see how it behaves." The epistemology of stratigraphy is based on an ontological approach of cognizing the layers in the section. By focusing on the behavior of the layer [the English phrase was always used] a dynamic agency is attributed to the layers in the section and science of stratigraphy is emphasized as the accurate observation of the behavior of these layers and marking them.

When the Trench Supervisor was sure of his reading of the stratification he would use his knife and mark on the surface of the section the various layers he had observed and studied, by scratching at the junction where two layers appeared to separate. The act of using the knife and marking each layer on the section was considered an essential practice for the archaeologist in the trench. An AA explained to me that: "these knife-drawn lines are used to help tell the story in the quadrant. If one has to convey any meaning about the trench, lines have to be drawn." Often students in the trenches were instructed (especially when they were unable to differentiate between layers) to draw multiple lines to depict one layer on the section: "be the master of these lines. For these lines are the basic referral points of the trench." Interpreting the stratigraphical section was not enough, it had to be inscribed and made definite. These lines were considered very meaningful by the ASI archaeologists, as they were the visual ascription of time in the trench. When the knife was run through the section, it's depth and boldness reflected the confidence of the Trench Supervisor in his interpretative

abilities. After the lines were drawn, the Trench Supervisor invited senior archaeologists during their inspection (AA, ASA, SA or even the site Director) to study the markings on the section. The senior archaeologists would then pull out their knives and read and interpret the stratification on the section. They would erase the markings the Trench Supervisor had made if they felt those to be incorrect, or re-scratch lines more deeply to emphasize the correctness of the markings of the layers. This process would be undertaken after discussions with the Trench Supervisor after a consensus had been reached. Within the hierarchical framework of the ASI, however, the consensus would be overlooked when it was the SA or the site Director who was inspecting the layers on the section.

Once the rituals of observation, inscription, inspection, and re-inscription were completed, the layers would be labeled. Labeling the stratigraphy on the section was considered to be a performative aspect of the excavation of trench analogous to the "preparation of the subject," a practice I discuss in detail in the next chapter. Each trench leader was given a series of white, square cardboard pieces on which stencils were made in dark ink, with numbers inscribed in a circle. These were the layer numbers, which were pinned onto the section to identify the strata. This practice had a sense of finality: "Once the layers have been labeled, then it means that excavation in the trench has ended, for the time being. Now the technicians – draughtsman and photographers can enter and do their work. Once they are done, then the excavation starts again," explained a Trench Supervisor to me. We were sitting cross-legged on the floor of a four-and-a-half meter deep vertical trench. The supervisor was directing a laborer, who was trying to pin the layer label on top of the trench by balancing on a bamboo ladder resting on one wall of the quadrant. This process of labeling was also very Wheelerian in nature. Wheeler spends a considerable time discussing the value and importance of labeling the stratigraphic layers of the section:

I like to see my sections plastered from head to foot with orderly arrays of labels, which serve three main purposes: they demand clear and decisive thought on the part of the supervisor who invents them, they show on the ground and on the drawing precisely what his small-find labels mean, and they make it possible for the director or a substitute-supervisor to understand at once the diagnosis up to date (Wheeler 1954: 72)

It was through this process of labeling that stratigraphy transforms *objective facts about layers into empirical evidence*. This practice of visually marking the various layers of stratigraphy is the definitive inscriptive process through which the layers on the section are accorded

empirical value. Such a process of tagging makes the layers factual artifacts of the archaeological excavation and creating various layers creates an objective category of temporality. In the process, the messy practice of reading, interpreting, and forming consensus is erased. Instead, the strata/layer as a concrete factual category of archaeological science emerges. Once this objective inscription in the trench was completed, the excavator would end their task and the ASI technical staff - surveyors, draughtsmen, and photographers - would enter the trench to transform the epistemic spatiality of the trench with its layers and material culture evidence into an objective representation medium - the processes and practice of which will be discussed in the next chapter.

In a "good" ASI excavation, before any horizontal excavation, a proper vertical trench at the top of the mound had to be dug in order to ascertain the stratigraphical chronology of the site. I observed that in most of the SHP sites due to lack of time, this trial trench was often not dug at all. At sites like Dholavira, where excavation was been conducted for more than 12 years, a number of vertical trenches had been surgically dug across the whole site— in order to ascertain the stratigraphical history of the site. The stratigraphical chronology in these vertical trenches then determined the "standard" for the whole site. On enquiring, I was told by the AA and the site Director that: "it is not possible to turn every quadrant into a vertical trench. You can only dig a couple and then take the data from these as representative of the site".

Since the visuality of the section was so crucial for observing the stratigraphy of the trench, a huge amount of manpower was spent in the excavation trench to make good sections. The importance of this skill was so great that each team of workers had an expert "section-cutter." This expert was a laborer who had a "good hand" [*accha haath*] in cutting a clear and defined section, which had to be "ninety degrees to the floor" of the trench, without any bulges or depression – "it has to be clear like a mirror" [*sheeshe ki tarah saaf*] explained a draughtsman to me. A site director, explaining the importance of a section cutter, informed me that since Wheeler up until the mid 1980s, there had been a separate category of skilled craftsmen working at the site who were given more daily wages than the lay workers - among these, one category was that of "section cutter." Although the section cutters, during the years of my ethnography, were not given higher wages, this category of workmen remained an essential feature at the ASI excavation work force. I realized the importance of the section cutter's role in Baror when an AA introduced me to a very elderly laborer working in one of the trenches

as a section cutter. He had been trained under B.B. Lal during the excavations of Kalibangan in the 1960s. He had been summoned from a village near Kalibangan when excavation work started in Baror: "only to cut sections and train other laborers to make proper sections." The job of the section cutter started as soon as the first dig took place and the horizontal wall of the gridded trench emerged. Preparing a section was a time consuming task and a section cutter would take at times almost a day just to turn the wall of the trench into a proper section. This practice was emphasized in Wheeler's instruction in his manual: "there are in practice various ways and means of dealing with the reluctant, sun-baked section of the Orient, or indeed with many sections in the West. Damping, and careful scraping with a knife or turf-cutter, will often provide the remedy by bringing out more subtle variations of color or material" (Wheeler 1954: 60). Trench Supervisors and the AA would make it a point in their inspection round to see how the section had been prepared. It is important to note that although some AAs I observed were very good section cutters, it was not expected of the students to even learn section cutting. The task of cutting the section was the responsibility of solely the subaltern laborers at the site. However, often the Trench Supervisor or an AA would jump into the trench and pull out the knife from his pocket to sharpen the section. I was also often told that to be a good archaeologist it was important to be a "fine section cutter." But, neither in the training nor in the daily practice of ASI archaeology, did I ever see section cutting being done by any of the officers. Within the hierarchy of work [*kaam*] on the excavation site – section cutting was relegated to the domain of the laborers although it was considered "skilful."

For ASI archaeologists, stratigraphy was considered to be the most technical artifact of the archaeological excavation in that it required expert knowledge. I observed that in the hierarchy of knowledge desirable for an ASI archaeologist to have, it was the ability to read stratigraphical layers of the trenches that ranked the highest. It was not the ability to recognize artifacts or the skill to excavate structures in the trench, but the ability to decipher the various deposits of the soil that were evident in the section, and demarcate the chronology of the trench on the basis of stratigraphy that was considered to the specialist skill of an archaeologist.

Recognition in the Trench

The practice of discovery in the ASI archaeological trench was symbiotically linked to the idea of recognition. Archaeological discovery theoretically cannot be possible unless the

excavators are cognizant of what they are looking for. Archaeology, like any other disciplinary practice, is based on a systematic socialization process of scholarship and erudition – university education and degrees. In the ASI, scholarly training was essential to the making of an ASI archaeologist – formalized by enrolling for a Diploma in Archaeology at the Institute of Archaeology in New Delhi. Even students who had been trained as archaeologists at University departments across the country – with MA/ MSc degrees, had to have a Diploma from the ASI's Institute in order to join the ASI as an archaeologist. It was ironical that, with such a distinct idea of training and scholarly socialization, the primary excavator in an ASI archaeological site was an untrained laborer. These "excavators" who dug the trench, cleared and shuffled the excavated earth were "illiterate" [*dehati*] men and women. They were neither trained as archaeologists nor were they conceptually conversant with the goals and objective of the archaeological excavation. During my ethnography I observed that the actors at the site who did most of the physical work of excavation were the laborers, and not the Trench Supervisors or the archeologist officers of the ASI. "That is what they are supposed to do, manual labor – dig trenches, remove dirt and carry excavated earth [*kaam-kaaj – trench khodna, mithi uthana aur phekna*]," said an ASA. Paradoxically, these "manual laborers" were the first and foremost actors who engaged with an artifact or a structural feature in the trench. In the social hierarchy of the excavation site, they were the lowest. However in the hierarchy of discovery, they were the primary actors. These were the first to discover an artifact in the trench, to notice something unusual and valuable –and to recognize its epistemological value.

Once the quadrant had been inscribed on the landscape, the work of the laborer-excavator was to conduct the "first dig" – six to ten inches in depth. Both men and women did this with the help of small pickaxes [*choti gaithi*]. After the ground had been unearthed, the laborers, with their bare hands or with trowels, brushes, or even pieces of iron scrap or plastic, would rummage through the excavated soil and search for artifacts. It was during this process of searching that the laborers and excavators would find objects that they presumed to be valuable – potshards, animals bones, precious stone objects, beads, micro-beads, and terracotta objects: "...anything they thought was unusual, they were told to pick it." The potshards and the bones were very easily identified by them and were kept in separate tin containers. Other objects would be handed over to the Trench Leader or to the Trench Supervisor and kept separately. Fragile artifacts like carnelian beads, metal objects, and terracotta figurines would immediately be wrapped in cotton and stored in a polythene bag or a plastic container by the

Trench Supervisor. Findings at this stage were only labeled with their layer number, depth, and the name of the object. Since the objects were found during the process of searching through the debris after excavation, three-dimensional recordings were not conducted. Only in the case of known precious Harappan artifacts such as seals with inscriptions, metal projectiles, complete terracotta figures, and complete carnelian beads, were three-dimensional recordings undertaken. But in such cases, only approximate recordings could be taken, as it was impossible to pinpoint the exact location of a given find. Precise measurements were only taken when the excavators had discovered a habitational floor, which I will describe in detail in the following sections of this chapter.

The act of discovery and recognition was a "routine act" carried out in every trench and by almost all the laborers who worked in the trenches. The laborers were constantly recognizing and discovering various kinds of artifacts, structures, and material culture. The possibility of a laborer who had never been trained as an archaeologist, to do a specialist archaeological task, was intriguing. However, my informants—both the laborers themselves and the ASI archaeologists, considered it common sense and were surprised when I asked them about this process. One Site Director was visibly irritated and in a solemn tone intoned: "We train them. It takes weeks and days of very carefully explaining to them what was essential and what was important. Look, these are hard working people of the earth (*matti ke log hai, bhai).* They know when something is unusual and different. They learn very fast. We show them photographs of antiquities that are usually found in the Harappan sites. We carry a sample of potshards, steatite beads, and other commonly found artifacts to show them. And they just get it. And there is no space for mistakes for there are also the Trench Supervisors, AAs, and the Technical staff. Everyone is in the trenches. So nothing gets lost or goes unseen. We have complete control." For the Site Director, the key issue was that: "...they just have to be taught and explained that these things are important and that we are searching for these objects. And then they do it." The process of recognition is common in the discipline of archaeology –a matter of epistemic socialization– through photographs, and sample objects. In the ASI, the process of discovery and recognition was controlled and regimented, with the laborer-excavator given the agency of identifying objects and not of discovery. But their epistemic agency was only valid until the moment of recognition, after that it was the job of the archaeologist "...to make the finding scientific. By measuring them, labeling them, cataloging them and finally writing the reports," explained the AA, differentiating between the work he

did and the task the laborers undertook. In the ASI site, this division of epistemic labor was assiduously followed – the laborer dug and discovered; the archaeologists gave the discovery its scientific value. As I show later in this chapter, artifacts were deemed "discovered" only when mediated by a process of labeling and recording. The agency of their discovery was attributed to various official actors at an excavation site through a chain of credibility and authority.

At Baror, an ASA explained to me: "Look, their village surrounds these archaeological mounds. Day in and day out they live with potshards scattered around their field and villages. So they know all the antiquities that we are looking for. It is a part of their lives. Harappan mounds, like most sites in India, are not hidden feet and feet under the earth; they are very visible mounds, with artifacts scattered hundred of meters across the landscape. The mound and its antiquities are part of their lives. So these people are the best persons to dig. For they know the site better than us. We don't even have to train them. We just tell them to pick up potshards for us. That's all." Such a response was representative of many archaeologists and was very often utilized to justify the employment of the local population as workers at archaeological sites. At Hansi, which was a monumental mound with a small town surrounding the site, an AA explained: "This is, after all, their land. They know this area the best. We just make the act of finding things more scientific." I did observe that in many cases the local laborers were able identify artifacts much more efficiently than the ASI archaeologists. Most of the ASI archaeologists acknowledged that that some of the laborers were far superior field archaeologists than them. For example, a AA at Dholavira who had worked at Rakhigarhi as a student described: "at Rakhigarhi, which as you know, is the largest Harappan mound in India, the locals were already expert diggers by the time we came; there was a clever [*chaloo*] primary school teacher who knew the value of the mound and its antiquities and would pay a few rupees to his students to collect seals, beads, and other antiquities. You will not believe it, but when we reached the site, we also asked those same students to do the primary exploration work for us. They were so good that in the first few days we got a few seals and many carnelian beads. We were not allowed to employ them as we are a government organization. We did use the children to help us during the excavation." Although I do not have any ethnographic evidence to support this anecdote, during my daylong visit to Rakhigarhi with a group of students from the Institute, we were accosted several times by a bunch of kids cajoling us to buy beads and terracotta figurines that they had

collected. When I asked them how they had found these artifacts, a young boy remarked, "after every rain. A lot of these objects are scattered over the surface. We just collect them. Every one does. When foreign tourists come they give us a lot of money"

Once I was observing daily work at a trench. I was sitting with an ex-student Trench Supervisor. This was a new trench in the Lower Town of the Dholavira mound. The Lower Town was a sprawling habitation deposit consisting of formidable structures – homes, shops, elaborate drainage lines, and streets, which the excavators considered as inhabited by the working-class members of Dholavira during Harappan times. It was the lowest habitational deposit at the site, much lower than the citadel surrounded by the fortification wall and the middle town. The trench was inside a domestic habitation structure and the excavation in the trench was attempting to locate the working floor of the room. This was the second dig in the quadrant, and it was about eighteen inches deep. Half of the quadrant had been excavated and three elderly women were sitting on their haunches, shuffling through the excavated earth looking for artifacts, while two men with small pickaxes were digging in the northwest part of the trench. Slowly, one of the women, who had been meticulously rummaging through the excavated earth with a trowel, gathered some dirt in her hands and walked towards us. She came and opened her palm and showed it to the Trench Supervisors exclaiming " Sir, see [Sir, *dekho*]." In the dirt were about a dozen or so micro-steatite beads. "Very good! very good! Find more [*bai, bahut accahe! bahut accahe! Aur khojo*]," the ex-student supervisor excitedly replied. Pleased, the woman went back to her corner in the trench and started to rummage through the excavated earth again separating the dirt with her dusty fingers. "Look at her. See, she is going through the dirt as if she is removing the gravel from the rice [*chawal se kankar nikal rahi hai*]. That's why you need local labor," he explained in a very satisfied voice. "Will you ever to do this? These are micro-beads. I will never be able to find them in this much of dirt. This is even more difficult than finding a needle in the haystack. But these women do it every day. Look, they have the in-built training. They have been removing stones from tons and tons of rice all their lives, so to find beads in this dirt is natural for them." I found this narrative representative of the way most ASI archaeologists would justify the usage of local labor at the site. Local labor was important in the archaeological site for the ASI because it not only provided the work force to clear the vast tract of land that an ASI excavation site necessitated, but also because it possessed some innate skills that were "perfect" for the excavation process.

For the laborer digging in an ASI trench, the practice of recognition and discovery was equally based on common sense. However, there was a distinction in the answers that I got from laborers working at sites like Baror, Bhirrana, or Hansi – where excavation was in the first or second season, compared to Dholavira – where the ASI had been conducting excavations for more than a decade. At the former sites almost all would respond "Sir, this is what we have been told to do. So we are doing this." Some of them would say that they had known these mounds since they were kids: "I see these potshards and bones everyday. I know them since I can remember. We would take our animals to graze here. I never knew that these potshards could be important for anyone. When I started work here, the sahebs told me to look for these potshards. It is very easy for me," explained a teenage boy, from a village near Baror. He biked ten kilometers every day in each direction to work at the Baror site and noted that: "it's even worth dropping school for a month" – he was studying in high school in the sub-divisional districts headquarter of Anoopgad (District Ganganagar, Rajasthan). For most of these laborers, working in the archaeological trench amounted to doing what the sahebs told them to do. Archaeological work was conceived as plain work, which was instrumental in getting them their "bread and butter" [*rozi roti*]. However, archaeological work had a different meaning for some of the senior laborers at the site of Dholavira – especially those who had been involved at the site since exploration and excavation and had worked at the site since the late 1980s.

Dhamji bhai – one of the first laborers to begin work at the site of Dholavira, was now a supervisor in the pottery yard. When asked how he had learned about the different kinds of ceramics at the site, he explained, "Sir, I learned all about archaeology and Dholavira by following and observing Bisht sir. When he first came, I was one of earliest laborers at the site. He taught me all I know about archaeology. I learned how to dig with him –with both big and small pickaxes. To clear the earth with a brush, prepare subjects, and to use a knife properly on the floor and the section. I did not know anything: he taught me all of it. But the majority of what I learned was by observing Bisht sir. I would just stick to him. If he were in the trench day and night, I would be with him without food or water. I would carefully see how he did things and I just picked up." Dhamji bhai did not dig in the trenches anymore. We were sitting under a tree overlooking the massive Dholavira pottery yard, where a group of women was sitting with their duppattas protecting their faces from the harsh, March sun. With

heads bent down, they were washing the potshards with water and toothbrushes. As we spoke, Dhamji bhai rebuked them casually if he heard the slightest murmur: "Stop chattering *bais*. Do your work. This new saheb is sitting here. He is observing all of you. If you talk too much, he will take your name off the muster." Nonchalantly, he continued, " Sirji, it was matter of survival. I had to learn things that Bisht sir, and other officers did not teach. I knew that the ASI would work in Dholavira for a long-long time. So in order to get a job, I had to learn everything. And it is because of my knowledge that in a few years I was made permanent." He was indeed, one of the selected few senior laborers at the site who did not dig, but worked as site supervisors. He oversaw the laborers working in the pottery yard. "Today I can tell you the difference between pottery from the earliest level in Dholavira to the latest phase," he proudly told me. Another day during my daily visit to the pottery yard, I was asking him about the various kinds of ceramics found at the site: "Something I must say, Sirji, please don't mind - even these young AAs with a degrees cannot tell the difference between various kinds of pottery. All these are just kids fresh out of school [*kal ke chore hai*]. We have to listen to them because they are officers. But sir, they know very little about the site or even about the Harappans," confided Dhamji in slightly hushed tomes although there was no one within earshot. For senior laborers like Dhamji bhai, recognizing ceramics at the excavation site was both a matter of skill and pride. He had earned his archaeological skills as a survival tool, like a lot of senior laborers at the site. Working at the ASI site was not just a means of earning bread and butter but it was also a way to gather knowledge about the site. During the non-excavation season, senior laborers like Dhamji bhai would also work as tourist guides for the few tourists who would make the long journey from Bhuj or Ahmedabad to visit the site.

Vala bhai, another senior laborer at the site had the reputation of being the unsurpassed "bone-expert" at the site. When I first came to Dholavira, I enquired among the ASI archaeologists working at the site if there was any zoo-archaeologist at the site. An AA apologetically told me that in the ASI, animal bones have a secondary value: "The ASI does not have any zoo-archaeologist on its payroll. There are experts who are invited by Bisht on and off. But there has not been anyone permanently. The only person at the site who knows bones like the back of his hands is Vala bhai." Vala bhai was a lower caste farmer from the village of Dholavira, who along with Dhamji bhai and four other senior laborers at the site, was a full-time chowkidar. They were daily-wage laborers who were employed by the ASI year-round as site custodians. During the months of excavation, they worked as site supervisors and did various

specialized jobs – from recruiting laborers from near by villages, to doing excavation and conservation work. They were all respected by the laborers and technicians at the site as well as the officer archaeologists. When I asked Vala bhai, during one of my numerous conversations with him, how he became the "bone-expert" [English term used], he explained, "Sir, it was a coincident. I don't remember exactly, but it was after two or three years of excavation that I learned about bones. Once a white sahib from your America came, and Bisht sir asked me to be his assistant. He was very kind. He taught me everything I know today. He was just here for few weeks, then his student – a madam worked at the site. She was also very nice and I worked with her as her assistant whenever she came to the site. She also taught me a lot. But she has not been coming to the site for the past few years. Now I look at the bones. But sir, I have only studied till class five. What research I can do? That, you have to do. I can only tell which bone belongs to which animal – cow, pig, and goat. But I cannot do anything more than that. No one here knows about the animal bones. The Officers and technicians don't know a thing. I am the only one who knows - so they call me bone-expert. Really sir, I am no expert. This is what they just say. But if I have to work here I have to know all this. It is a matter of survival" For Vala bhai and Dhamji bhai, the ability to recognize pottery or animal bones at the site was a means to not only earn money but also a way to make themselves useful and important to the site. One evening, standing on the citadel overlooking the Rann, over the setting sun, Vala bhai remarked, "after all sir, it is all about money. If it was not for this site I would have been struggling in the city of Ahmedabad working as a coolie."

Dhamji bhai and Vale bhai were exceptions. There were very few laborers like them in Dholavira, and none at all at the newer sites. "Skilled laborers," as both these experts were called, were not many on ASI sites and existed: "only at sites," I was told by a senior ASA, "which had a history of long excavation. I remember there were many like these in Kaliganban." This ASA had started his career as Technical Assistant (earlier designation for an AA) at Kalibangan working with B.B. Lal and B.K Thapar. "They played a very important role in the day-to-day workings of the archaeological site. They were local and understood our objectives as archaeologists. They were very helpful in training other laborers and getting work done," explained the ASA. But most laborers at the sites were unfamiliar with the practice of archaeology and their role in discovery and recognition of artifacts was very limited. The epistemic role of the laborer in the trench was restricted to providing an initial marker to the object discovered as material culture. They were the first to recognize artifacts

and structures. The information then moved to the next level of expertise, which added credibility to the primary recognition of the laborer and it was only then that the material culture was considered officially discovered. The movement from recognition to discovery involved a chain of credibility or a hierarchy of authority that could establish that the artifacts recognized by the laborer were indeed discoveries. In the ASI trenches, the laborer's cognitive and conceptual skills were only employed to unearth the artifact from the earth and recognize its value. They did not discover artifacts. For an artifact to be discovered it had to have its significance ratified by an authoritative and credible member of the archaeological project. The mere act of unearthing did not transform the object into an archaeological artifact worthy of epistemological intervention.

Measuring and Recording

The excavation in the ASI trench was framed by two planar surfaces: the horizontal floor of the trench and the vertical section of the trench. Through the co-relationship between these two surfaces, evidence is produced at the site. By triangulating an artifact or a structure between these two surfaces, the discovered material culture is transformed into archaeological evidence. These are not static surfaces, they are constantly evolving and being shaped in the trenches. With every dig, a new horizontal floor is uncovered and a longer section is exposed. In the ASI trenches, the science of archaeology is located specifically in the ability to observe and record both these surface features as accurately and precisely as possible. The micro-practice of science in an archaeological excavation is the process of uncovering each horizontal layer as systematically and precisely as possible and co-relating the material cultural on the floor to the stratigraphic layer on the section.

In ASI trenches, very clear sets of rules and regulations were followed to optimize the scientific process of archaeology. These rules and regulations of scientific archaeology not only emerged from a collective wisdom of previous archaeologists who had spent their life digging in Indian sites, but also by following and regurgitating Wheeler's method. Although the general sets of norms were conventions learned through textbooks that were used in the Institute, these processes were emphasized on a daily basis to the Institute students learning the field method: "Archaeology is not a bookish subject. It is a field science. What you will learn in the field you will never learn in the classroom." These methods and practices were given to students during their training in the excavation site. During my fieldwork, I spent a

considerable time observing how senior archaeologists trained the Institute educated students in the "ASI's ways of digging," – which I was always reminded was different from "the way you dig in Deccan College." The training was primarily imparted through lectures by AAs and the Technical staff, and sometimes by the site Director. They were held in the excavation sites, trenches, and also in the evening after the excavation in the "antiquity class." It was during these lectures and classes that the students were trained not only how to dig and to read stratigraphy, but also how to measure, record, and write detailed notes in the site notebook.

"Archaeology is not just about discovery. It is about knowledge. If you just dig and remove objects without recording and measuring, that is murder. It is like killing the site. You must measure artifacts – where they were found, label them, and write notes about them. That is why you are given a trench notebook, and measuring tape," lectured the senior AA, to a group of students in a trench in Baror. This was one of the first days of fieldwork for a bunch of students from the ASI Institute who had come to Baror a couple of days before for their "ninety day" mandatory field training. We were all standing in a quadrant, as the senior AA at the site was explaining the importance of measurement in archaeological excavation, "precise measurement and labeling is essential for each and every antiquity that you discover, each structure you expose. If you don't label them or take measurements, then the site director will never be able to write anything about the your trench in the report. So, if you want your work to feature in the report, measure and label everything properly and write detailed notes in your site note-book." Then he proceeded to demonstrate how each artifact that was discovered in the quadrant was measured: "We usually follow 3D measurement—which means that you triangulate and then measure the depth." He then summoned several trained laborers and showed the students, with their help, how 3D measurement should be taken. The AA continued, "...so take the two quadrant pegs, which are the closest to the antiquity. And take the horizontal measurement using a string, spirit level, and the measuring tape. Then, take the vertical measurements using the plumb bob." It was through this practice of measuring artifacts, structuring the process of discovery in the quadrant and labeling and making a record about them, that archaeological evidence was transformed into archaeological knowledge. Solely the discovery and recovery of the artifact does not make archaeology a scientific practice. It is only through the process of fixating the artifacts and structures found in the trench within the larger gridded matrix of the site, that archaeological science is performed. The "science" is in the ability of archaeologists not only to dig a trench correctly following the

Wheelerian Method, but also to record and document the excavated artifact in such a systematic way that once the excavation is completed, the notes and recordings formulated can be easily summoned when creating a narrative of the site. During lectures, the Institute students would be told regularly, "Archaeology, as we all know, is a destructive process. The need for precise recording and measurement is most crucial. If you don't do that, then you are no different from gravediggers and thieves. We are archaeologists because we work systematically." For the ASI archaeologists, the systematic process of recording and measurement was the specific practice through which the science of archaeology was carried out in the trench every day.

Not each and every artifact discovered in the trench was measured in the way explained above: "Only the antiquities have to be measured and recorded the way I have shown. For pottery and bones only measure the depth," clarified the AA to the students. For the ASI archaeologists, the conceptual architecture that framed their archaeological intervention in the trench is revealed in the way in which the AA differentiated between pottery, animal bones, and antiquities in the above statement. Archaeological intervention, for the ASI was not the retrieval of material culture as archaeological artifact but as archaeological antiquity. This difference between artifact and antiquity was a crucial component of the archaeological lens through which material culture was conceived by the ASI archaeologists. Antiquities referred to all artifacts that the ASI archaeologist believed were "precious"—such as fragments of metal objects, terracotta figures, shells, objects, beads, and other similar artifacts. "These go into the antiquity trunk," explained an AA at Bhirrana who was "in-charge" of all the antiquities discovered at the site. "These are precious and rare objects and very different from the pottery and bones that are found at the site. These are the objects that are eventually photographed and drawn and finally only a very few are finally published and exhibited in the museum," he explained. "whenever that happens, of course, which is also rare," he noted sarcastically referring to the culture of non-publication of site reports in the ASI. Thus, the ASI archaeologists differentiated between rarely found antiquities and the prolifically found artifacts like ceramics and bones. At the level of daily practice this conceptual difference was reflected in taking 3D measurements of only the objects that were considered antiquities by the excavators, recording them in the site notebook, and individually labeling each item. Ceramics and bones, on the other hand, were lumped together by the depth and layers in which they were found. Along with the 3D measurement of each antiquity, the recording

process consisted of labeling each item with detailed tabulations mentioning site, object, date, trench, quadrant, stratum, locus, depth, period, and material. But ceramics were collectively placed in a cotton "pottery bag," and labeled by the categories trench, quadrant, stratum, and depth.

Discovery and Chain of Credibility

I was sitting in a quadrant digging with a student trench leader in Baror. There were about twenty trenches that had been opened up for excavation in the lower part of the mound (in 2004), which had been destroyed by prior agricultural activity. So the ASI archaeologists leading the excavation had decided that the destruction of the upper layer of the site "should be exploited" and excavations were conducted to examine the Early Harappan layers. An AA explained the logic of this archaeological intervention when I first came to the site: "Although this site is partially destroyed, it is still very useful. A local farmer had dug open a part of the mound, as he wanted to expand his agricultural field. But the same season his brother died. This was a bad omen. He decided to abandon his plans of cutting through the rest of the mound and expanding. This has been a godsend for us. Although he destroyed the upper layers of the site, the lowers layers are still intact because no regular agricultural work was done. So now it is faster for us to hit the Early Harappan levels here. You know, in sites like Surkotada and Dholavira, early Harappan layer is nearly four to five meters deep. Here we can reach it in a meter or two." This trench was about a meter and a half deep and the excavation was done through "slow and small digs." The trench leader, who was an ex-student, explained: "These trenches are very barren and other than some Hakra ware and animal bones, nothing was discovered." While I was working on the excavation with the team – digging with small pickaxes— a laborer came and told us that he seemed to have found some mud bricks in his quadrant. Excited, we proceeded into the quadrant. The laborer was a section cutter, and he pointed out distinct mud brick alignments that could be seen, about eighteen inches above the surface of the trench floor. The Trench Supervisor asked the laborer to take over our work in the quadrant we had abandoned and he then pulled out his knife and started scraping the section slowly and delicately. After a few minutes, he noted, "Can you see it?" and proceeded to mark with his knife the outline of a mud brick on the section. He continued, "This is indeed a mud brick wall. But look, it has been dug through. The student excavator was unable to notice this and dug through the whole wall." The Trench Supervisor was referring to the students of the Institute who had been digging this part of the mound for six weeks and now

had moved to another part of the site. "This has to continue on the floor," he observed and then knelt down and proceeded to scrape the floor of the wall and soon it was possible to see the mud brick wall that he had mentioned. There was a distinct color change and to make it more obvious he again used his knife and marked on the earth the mud brick wall. "We have to call the AAs for inspection," and called one of the laborers to go to the camp and call a senior officer. After a few minutes, one of the senior AAs at the site arrived and he jumped into the trench and inspected the area where the mud bricks had been discovered. He also used his knife to re-mark on the surface the lines of the mud brick, and then told the Trench Supervisor, "clear up the subject. This needs to be photographed before any further excavation. And please note the measurements carefully in the trench notebook." By the end of the day, all the other AAs, technicians, and Trench Supervisors had come to observe the mud brick wall in the trench, Finally the SA who was responsible for the entire site also came and noted the discovery. In the evening, during the Institute antiquity class, it was announced to the students and all present in the camp – that the team had finally found a mud brick structure in the "early Harappan part of the mound."

In the case of the discovery of this artifact, as I have shown above, it became credible only when the trench leaders ratified the discovery made by the laborer and then labeled the object "discovered". There was a "chain of command" at work, as a junior AA referred: "we are just cogs in a well oiled machine where there is no room for disturbance." The idea of systematic work and scientific work go hand-in-hand for the ASI archaeologists, and it is considered part of a process whereby an artifact first recognized by the laborer and measured by the Trench Supervisor is deemed a discovery by the whole community of archaeologists and technicians working at the site. "The ASI excavations are so systematic that other governmental agencies are awed by our scientific methodological streamline way of doing things when they come to our sites for visits," remarked an AA while we were working in a trench in Dholavira. He continued: "Once a bunch of junior army officers came to the site. This was during the Indo-Pak build up in 2001-2002. They had just graduated from NDA. I took them around the whole site. They were very inquisitive and so it was fun. I gave them a tour of the site for a whole day. And at the end of it, they were so impressed that they told me: 'You guys work like us in the army. Everything is so perfect and organized.'"

The distributive hierarchy of work at an ASI site is very clearly demarcated. The laborer is

assigned the hard and menial task of digging the trenches. This is considered the easiest trade to learn and master. The act of recognizing and discovering antiquities and structures is the next level of expertise. This is considered to be an essential skill in the micro-practice of excavation and is also performed by the labor force at the trench, though under the guiding eye of a trench leader, who usually is an AA—an ex-Institute student or a student archaeologist. The next level of hierarchy involves expertise in reading stratigraphical evidence of a quadrant and relating it to the trench floors that have been excavated. Only senior archaeologist officers of the ASI can do this task with certainty, although as part of their training, student archaeologists are also encouraged to identify and read stratigraphical sequences.

Then comes the ability to create typologies of artifacts discovered, to divide them into classificatory indices, and finally to co-relate the artifacts discovered with the stratigraphical evidence of each trench and design a chronological narrative of the whole site. This is considered to be the most complex task assigned to the senior-most members of the ASI archaeological team — led by the site director, about which I will discuss in detail in the next chapter. The ability to excavate, recognize, and discover is the lowest in the hierarchy, mainly because, as an AA explained, "To recognize is not difficult, as one studies numerous books and learns what the various types of Harappan pottery are, or what a steatite bead, or lapis lazuli looks like. But learning what layers mean on the section, and what is the relation of layer 32 to a burnt floor, is a matter of practice and experience only. You cannot learn that in school. You need the experience in the trench for it. You have to know how to use your knife and dirty your fingers. There is no other way."

The process of discovery on an ASI site is a process of validation. It has to be done by various members of the excavation team. The linear movement from the moment of recognition to the moment of discovery is also a development from the laborer who works in the trench to the Trench Supervisor who is the first to ratify the discovery by recording and labeling it, to other members of the archaeological team. The discovered object – an antiquity, structure, or a stratigraphic layer, gains epistemological credibility only after its existence is ratified by a chain of authority. The laborer unearths the artifact, the trench leader measures and labels it, and consigns it to the realm of epistemological conscriptions, whereas as the senior members of the ASI team who study these artifacts finally tie them into the narrative of the site as epistemological evidence. This transformation of the archaeological artifact into

epistemological evidence by employing representational devices at the excavation site will be the focus in my next chapter.

Conclusion

The emphasis of this chapter was to shift the focus of my ethnographic intervention from the archaeological site as a whole, to the production of archaeological evidence. I have described the process by which material culture in the archaeological trench was recognized, discovered, and transformed into archaeological evidence. The ethnographic observations and analysis shifted from the excavation site as epistemic spatiality to the trench as the epistemic box. In this chapter, I concentrated on how the micro-process of scientific archaeology was practiced in the context of the archaeological trench and quadrant and how evidence was recognized, discovered, and produced. I minutely demonstrated how ASI archaeologists and workers in the trenches recognize and discover material culture, name it and epistemologically fix it. I investigated their process of recording artifacts and structures at the site, as well as relatively dating the deposit by correlating it within the stratigraphical matrix of the trench. In the next chapter, I will show how the object's discovery and transformation into evidence is turned into archaeological knowledge. The basic theoretical architecture that frames the archaeological production of the knowledge of the ASI is that of culture-history archaeology. This framing is responsible for transforming the evidence produced in the trenches into archaeological facts that are finally consumed by the larger scholarly community of archaeologists. It is important to note that archaeological process and practice is a destructive act, which results in the destruction of the very evidence, which archaeologists look for. All that remains is excavated material culture and the excavated site as evidence of past human activity. Central to this form of scientific practice is the creation of a complex system of processes and practice of representations that are instrumental in transforming the evidence produced at the site into archaeological knowledge. The next chapter in this dissertation will focus on this process of archaeological practice, most of which is carried out at the archaeological site.

Chapter 6
Politics of Representation

Introduction

The ethnographic focus of this chapter is the key post-excavational practices and processes of the ASI archaeological intervention at the site. The excavation site was not just the location for production of archaeological knowledge but it was also the setting for the performance and representation of archaeology as a statist enterprise. Along with the epistemological operative of excavation, ASI archaeologists actively engaged in the performative imperative of presenting and (re)presenting the evidence they unearthed. This presentation was simultaneously aimed at the epistemic articulation of the archaeological site as an ideological location of state performance and the representation of excavated material culture as scientific evidence. These performative and representative practices were conjoined in the production of archaeology as a scientific project of the state. The exposed spatiality of the excavation site was regularly put on display for visiting state official and local elites. During these performative rituals, the excavation site would be exhibited as the location of state's power and appropriated to legitimize the ASI's intervention. The excavated site was transformed into an arena of spectatorial performance where the precision of scientific archaeology along with statist power was exalted in the uncovering of the ancient civilization that made up the nation. This spectatorial ritual was an essential post-excavational process of the ASI archaeological intervention and it deployed the performative aspect of the excavation site to further the ideological and epistemological goal of the statist organization.

The process of documentation and representation of archaeological excavation would proceed concurrently with the excavation, as described in the chapters above. The representational practice was motivated by the simple idea that archaeology was primarily a destructive process and therefore it was necessary to document the discoveries as an essential part of the excavation practice. In the ASI, there was a heightened awareness about the need to document exposed archaeological sites through photographic representation along with hand-drawn representations – diagrams and maps. The thrust of these representational systems was a spectatorial imagination, which attempted to present the excavation as a formidable scientific artifact in possession of the state. Epitomized by the rituals of site visits, subject preparation, photography, and the excavation report, I argue in this chapter that the state and its fetish for

superficial perfection subsumes these daily practices of the archaeology.

The site visit

We had all been patiently waiting since morning. We had been informed that the VVIPs would arrive by 10 am. It was noon and there was still no sign of them. It was getting hot and the desert sun was blazingly harsh even on this mid December day. We were in the Director's mud hut in Dholavira – all the Institute students, ex-students, AAs, technical staff, the co-director, along with the SA of the Baroda Circle and his staff who had just come the night before for the VVIP visit. Sitting on red, yellow, and white molded plastic chairs, sipping endless cups of very sweet tea and munching Parle biscuits, served by a few senior laborers still in the camp - we were just "doing time-pass." Today had been declared a holiday because of the visit so the site had no laborers other than the chowkidars.

It was a big day for the villagers of Dholavira - for the first time in the history of the village, the Chief Minister of Gujarat and senior national level politician would be visiting the village. "Sir, the closest we have ever got to such a darshan of a politician was at the visit of Rajiv Gandhi - who came to Rapar during an election campaign many years ago," remarked a senior laborer in a loud voice, making all of us listen to him. While waiting, and generally chatting about banal matters, the SA, nervously drying the perspiration over his forehead with a white handkerchief, remarked, "These VVIPs - they are never on time. There is no difference between a film star and these politicians. They are always late." Soon a flurry of anecdotes about different VIP visits were flying back and forth - related by the AAs and the technical staff – minor politicians, district magistrates, MLAs, MPs, District Police Commissioner, Secretary of the State, their wives, daughters, and relatives, "Everyone wants to see an archaeological excavation. They think it is a circus or a zoo where everything has to be seen and touched. Too much of Discovery Channel and National Geographic Channel has made them think that they will see gold jewelry and mummies here" remarked a senior AA exasperatedly, "for their touristy fun we have to sweat." He continued in a similar vein, making no effort to to hide his disgust with the idea of the site visit: "it is not just a waste of our time, it is also a sheer waste of money and energy of the government." No one raised an eyebrow at this outburst; we were all tired waiting for the VVIPs.

It was after almost another hour of "time-pass," that we heard the sound that we were all

anxiously waiting to hear – the distant hum of army helicopters. In no time, we saw two helicopters in the horizon – the CM and his entourage had finally arrived. The SA, the co-site Director, the AAs – all dressed in formal ties, blazers, and trousers (and perspiring incessantly) raced towards the helipad adjacent to the site with newly marked signs in fresh lime over a rugged landscape. The army had leveled the landscape adjacent to the site and constructed the helipad during the 2001 Indo-Pak build up: "nowadays it is only used when a Minster or any other VVIPs come to visit the site," I was once told by an AA during my initial exploration of the site. As soon as the helicopters landed, out came the Chief Minster of Gujarat – Narendra Modi and the late Pramod Mahajan – the high-profile BJP Rajya Sabha MP, ex-Minster of Telecommunication, ex-Parliamentary Affairs Minister and the General Secretary of BJP, along with their family members, state officials, and a slew of the famed Black Cat commandos – the security men dressed in ubiquitous safari suits, dark Ray Ban sunglasses, and armed with automatic guns. With this began "the mother of all site-tour," – as an AA standing next to me remarked sarcastically. We saw from a distance the SA, the co-site Director, the Sarpanch of Dholavira - one by one going forward and garlanding the leaders in the midst of a minor dust storm raised by the still-rotating weary fans of the two helicopters.

Since I came to Dholavira, I would often hear the ASI officers and the technical staff talk about the impending visits of Advaniji, and Jagmohanji, who were planning to come for site tour. Lal Krishna Advani was then the second most powerful man in the NDA, the ruling party - after the Prime Minster. He was Deputy Prime Minister and the Home Minster of India and was considered to be the PM in waiting. He was an incredibly powerful politician who represented the extreme Hindu fundamentalist ideology of the NDA government. He was also the politician allegedly responsible for leading the Ayodhya Ram Janma Bhoomi movement that had eventually led to the demolition of the Babri Mosque in the December of 1992. Jagmohan was the then Culture Minster of India under the NDA government - a retired bureaucrat. He had first gained infamy as the "Demolition Man of Delhi" during the Emergency years (1974-77), as he around leveling slums at the fringes of Delhi during a virtual reign of terror unleashed by Sanjay Gandhi - the politically active son of the then PM—Indira Gandhi. Jagmohan later served as the domineering Governor of the troubled state of Jammu and Kashmir in the early 1990s when the Islamic secessionist movement was taking root. It was because of his approach of explicitly employing the Indian army and statist brute force to bring down the secessionist movement in Kashmir that the BJP had courted him, and

he was soon contesting the Parliamentary election under their ticket. In the NDA government, he first held the portfolio of the Minster of Urban Development, an interest harking back to his days as the 'demolition man.' Then in 2001, during a cabinet reshuffle, he was made the Minster of Tourism and Culture, under whose jurisdiction the ASI was designated. According to most of my informants, it was the political will of Jagmohan that led to funding for the Saraswati Heritage Project. An ASA at Baror, who had worked in the DG office for a number of years, explained rather matter-of-factly: "All senior archaeologists in the ASI have right wing sympathies. They might not be RSS propagandists [*pracharaks*], but they are openly with the BJP. It is no secret that Bisht was always close to Guptaji (S.P. Gupta), and B.B. Lal, who, as we all know, are saffron archaeologists [*bhagwa archaeologists*]. The Saraswati project has always been on the back of the minds of the RSS people; it was perhaps secondary to the Ram Janam Bhoomi issue, but it was always important. So when the NDA came to power, Guptaji began seriously lobbying with Advani and Murli Manhor Joshi to get money for the Saraswati project. But there was no political will. It was only with Jagmohan that the money came. I don't know why or how. But it seems that Bisht was very successful in selling the project on its scientific and archaeological merit. And Jagmohan, in turn, was able to convince the cabinet of its heritage and tourist value." I heard this narrative from a number of my informants, and all attributed the funding of the Saraswati Heritage Project to Jagmohan's clout in the cabinet, Bisht's argument for its scientific merit, and Gupta's and Lal's RSS connection.

There was also another reason for Jagmohan's importance in the ASI. My informants often told me that he had changed the fortune [*kismet*] of the ASI - it was now getting its due worth. An AA in Dholavira told me in pretty explicit terms, why he preferred Jagmohan over any other Minister of Culture: "he really cares about the ASI. Look, he is an able administrator. It was because of him that Bisht sir became the JtDG. As you know, the post of JtDG, ADG, and DG are all in the hands of the IAS mafia. Their lobby is so strong that no Minster has been able to let them forgo this post. It is because of Jagmohan's power and also because he is an ex-IAS officer that he was successful in getting the Jt. DG post out of the hands of the IAS clutches [*changool*]. All earlier ministers treated the ASI as an annoyance. No one really cared about us. It was also because of Jagmohan that more than 80 positions for the post of the AA have been advertised. The DG office has been constantly complaining to the Ministry about the lack of officers in the ASI. But the earlier Minster did not care. It is only because of

Jagmohan that now the ASI is getting its due importance. Otherwise, who asks about archaeology in India? Jagmohan has changed all this. He now takes part in day to day working of the ASI. I was once working in Bisht's office for a few days and there was at least one phone call from Jagmohan's office everyday. I hope the BJP wins next year and he becomes the Minister again," prayed the AA. This kind of praise for Jagmohan was fairly common and I heard similar narratives from other officers and staff.

In this context, the news of Advani's and Jagmohan's impending visit to Dholavira in the company of Bisht was very exciting. A few weeks before I had reached Dholavira and at the start of the season's excavation, Jagmohan had already come once for a site visit. And it was rumored that he was now convincing Advani and possibly Vajpayee — the PM of India himself — to come to Dholavira. The visit was considered very crucial, as an AA explained, "Dholavira is the crown of the Saraswati Project. If Advaniji or Vajpayeeji visit, this will push the project into national prominence. The problem of funding still remains. Bisht and the others think that not enough money has been poured into the project. They think there is not enough national awareness about the project. So they want the DyPM or the PM to visit." But unfortunately this did not happen although the impending VVIP visit was a topic of conversation for several days and I would often hear rumors that the visit was going to occur in a week's time.

One morning, I was sitting in a trench talking to a senior laborer supervisor, in the large excavation of the Southern Reservoir, when an ex-student trench supervisor interrupted my conversation. He took me away to under the shade of a solitary tree adjacent to the site and asked me: "Did you hear? Neither the PM or DyPM are coming. Instead, the CM and Mahajan are coming in two days." This was both interesting and important news. Modi was not as powerful as Advani, but he was considered to be a regional satrap of the BJP, and a very powerful politician on the national scene. He was an extraordinarily influential figure in Indian politics and was considered to be the younger and more virulent face of Hindu fundamentalism in India. His most recent claim to infamy was his own and his government's central role in orchestrating the Gujarat pogrom in early 2002, in which nearly two thousand Muslims were killed. Pramod Mahajan was a senior leader of the BJP from Maharsahtra, a much milder figure than Modi or Advani, but often touted as a future PM of India in certain circles of the BJP.

It became apparent very soon that the visit of Modi and Mahajan was only secondarily to visit the site and the primary purpose was to inaugurate a new part of Dholavira called the Pandit Deen Dayal Nagar - this was a new village that had been constructed by the MP relief fund of Pramod Mahajan after the earthquake of 2001 when the majority of the villages in Kutch district had been destroyed. Pramod Mahajan had adopted Dholavira because of its archaeological visibility and his desire "to do something for such a great and the oldest village of the nation," The first part of their visit consisted of presiding over a village meeting with more than a thousand villages – where the keys to the newly constructed houses were given to the owners –, and inaugurating the new village Dholavira. This was followed by a long-drawn political rally, which began with a nationalist song, sung by schoolgirls who had come from the district capital Bhuj - around 6 hours away. They were dressed in blue and white school uniforms with pigtails tied with red ribbons, and the song was set to the tune of a popular Bollywood song – "*Pardesi, pardesi, jaana nahi*," from the film Raja Hindustani. This was followed by speeches about Gujarat's pride [*gaurav*] and the greatness of the Saraswati civilization, given by a host of local politicians ending with a very emotional speech by the CM - Narendra Modi. It was after this ritual ceremony of the state with the demonstration of political exuberance at the fringes of the country, that the site visit began.

First, the VVIP entourage was taken to the monumental Eastern Reservoir of Dholavira where the co-Director explained the significance of the site to a gathering of almost a hundred people - the VVIPs, their relatives, aides, and assistants, their Black Cat security personnel, the senior district level officials such as the District Magistrate, District Collector, the Director General of Police, and the Block Development O fficer, members of the Dholavira elite, and village officials from near by villages. The crowd was diverse and the co-Director attempted to narrate the importance of the site in as non-technical terms as possible. The site visit lasted close to an hour during which the entourage was shown all the key architectural features of the site - the monumental fortification of the site, the rock cut reservoir, the stone brick laden Eastern Reservoir, the houses on the citadel, and the main street of the middle town. During the course of the site tour, a couple of junior AAs would often jump in to add some minor details. They later confessed to me that they were unhappy with the way the site tour had been conducted – "it was very lackadaisically done. There is so much of drama in a site like Dholavira. Nothing came out. It is only on the site tour that these VVIPs are under your

command – one should totally exploit the potentiality." The site tour was punctuated by talk about the Saraswati river, as a central entity in Hindu civilization and mythology. It was explained how the hidden, subterranean Saraswati river sprang forth at this site and entered the sea close to Dholavira. This was a narrative I heard for the first time here, but it was perfectly in keeping with the prevalent logic of right-wing politics that Dholavira and the SHP were located in. Then the entourage was taken to the camp site where the "antiquities" discovered at the site had been displayed on a molded plastic table covered with a white cloth. Metal and stone artifacts tucked in cotton wad and securely sealed in plastic containers were neatly arranged here. The co-director described the importance of each object and explained its functions, and only the special guests were allowed to pick up and feel the objects. Then the important members of the VVIP entourage were taken to the Director's mud hut, where they were served tea and biscuits in "special" porcelain cups and plates and the photographic album was circulated among the visitors. The wall of the mud hut had virtually turned into a museum where large photographic blowups of the site in laminated frames were displayed. The site visit ended with a group photograph of all the ASI staff with the two VVIPS.

The "site visit" or the "site tour" constituted a dominant form of display and performance of postcolonial archaeology that was regularly enacted at the ASI site. An archaeological excavation was an event that captured the attention of local politicians, government officials, and local villagers who lived close to the site. Unfortunately, the sites I did my fieldwork in were located in very underprivileged and poverty-ridden parts of the country; so most of the local villagers engaged with the site as laborers rather than as visitors. But there was a consistent stream of visitors and curious onlookers at the site. The ASI staff at the site considered most of them as minor irritants and did not bother with their presence at the site. The term "site visit" was reserved for the occasional visit of a local state official or a politician, or in the case of Dholavira, also senior army officers. During these visits, the daily work at the archaeological site would virtually come to a halt, as the director of the site or a senior archaeologist would take the "official dignitaries" around the site. This visit would begin with a tour of the various trenches of the site. Depending on the interest of the visitor, the site visit would last for a few minutes to a whole day, and always end in the camp where the visitor would be treated with the customary tea and snacks and shown photographs of the site. In the case of special visitors, they would even be shown antiquities. The site tour was an important event during an archaeological excavation as it was during these events that the ASI

archaeologists would showcase the excavation site, both as a scientific enterprise and as the feat of statist intervention.

The site visit was an important representative ritual of an archaeological excavation. All over the world, site visits are considered to be an essential pedagogical process through which archaeologists relate the discovery of the site to local community. In the ASI, it was mainly a representative ritual of power and performance through which archaeologists performed their knowledge especially in front of other state actors; crucially, such a ritual would not be enacted for lay people visiting the site. In the eyes of the ASI, the local community was exhausted under the categories of the "literate laborer" or a "minor touristy irritant". More than often, I observed that the ASI archaeologists would not take any initiative in showing the site to local community members who would occasionally visit the site, but would go "all out" to please the local statist representative or his wife who wanted to visit the site. Because of their power, the statist officials carried symbolic valence that had to be informed of the nationalist endeavor that the ASI archaeologists were involved in. Also, it became gradually obvious to me that the statist officials were more important to the ASI archaeologists because, in their eyes, these officials were their class and governmental peers – and it was important for them to perform archaeology as a nationalist project. An AA in Dholavira once told me after a site visit he had conducted for a group of young army Artillery Captains and their subordinated soldiers, who were on their way to the Indo-Pak border: "it is very important to do a good site visit for army officials because they have to know that by digging the ground we are also serving the nation as much as they are. It is like an Assistant Archaeologist of the Archaeological Survey of India talking to a Captain of the Artillery Regiment of the Indian Army on equal terms," but then he sheepishly commented in a sad tone about the pittance he got as an AA (see chapter 2), "Although we both are of the same rank, and probably from a similar school and college, I dare not tell him my salary. He will then know my true worth."

Subject Preparation

Just before the impending visit of the VVIPs, it was decided by the co-director of Dholavira, on the basis of consultation with the other archaeologists at the site, that the excavation work should cease completely, and the trench leaders were instructed that "all time and energy should be devoted to subject preparation." The motivation behind this instruction was to "groom" [*sajna-savarna*] the excavation site for the visit of the VVIPs. Accordingly, for about

four days, excavation work ceased and all the laborers, trench leaders and the archaeologist officers at the site got involved in what I believe was the largest subject preparation of all times. The nearly two hundred and fifty laborers at the site were involved in cleaning each and every trench that had been excavated during the season. Trenches that had been excavated a few seasons before were also cleared of debris and broken balk walls. Excavation was stopped in each quadrant and all time and energy were utilized in cleaning up the dirt, making flat, sharp and neat section walls, and removing dirt. The goal of this effort was to take away the messiness of the excavation site and turn it into a "newly married bride" [*nayi-naveli dulhan*], sarcastically noted an AA as he looked around at the frenzy with which the whole site was being prepared for the visit of national dignitaries.

It was during this frenzy, which matched the preparation for an upcoming wedding ceremony, that I observed the aesthetic fetishization of the archaeological process, which, in the event of the official visit, had taken monumental proportion. One afternoon, along with the site co-director, I reached a "rich" habitational trench on the citadel, where numerous artifacts were being discovered at a consistent frequency. A few days ago, a large red slipware ceramic vessel had been discovered here. Since the excavation had stopped, the trench leader, together with her laborers, was involved in preparing the site for the visit. In her attempt at subject preparation, she had scooped all the soil from the vessel and had carefully kept it in a plastic container appropriately labeled. When we reached the trench for inspection, the co-site director noticed that ceramic vessel was completely empty. He was mildly irritated, and gently rebuked the female trench leader, who was an Institute student: "This is not a scientific way of digging or exposing antiquity. You should only remove half the earth from the vessel for analysis and leave the rest of the earth in vessel. Dig in such a way that a vertical section is exposed inside the vessel. That is the scientific way of exposing." However, since the trench leader had scooped out all the soil from the inside of the vessel and kept it separately for analysis, the co-director instructed her to now obtain "similar looking earth" [*vaiasi hi mitti*] from elsewhere and put it in such a way inside the vessel that a vertical section would be visible. "It is important that they learn the importance of presentation" he remarked later, as we continued on our inspection of the trenches. He continued in a grave tone, as we walked down the steps of the fortification and went towards the middle town of Dholavira: "students should learn not only to dig in a scientific way but also learn that archaeological excavation has to be done aesthetically. Excavation is not just about digging for antiquities and structures.

Presentation is also essential. You see, along with the CM, there will be other archaeologists coming from the Baroda circle, and even Bisht sahib might come. And if they see that the subject has been dug incorrectly, they will laugh at us. We have to do a little bit of performance" [*thoda bahut to dihkawa karna padata hai*]. Such fixation on subject preparation was not visible only when VVIPS came to the site, but it was a regular occurrence at the site, even when a minor dignitary came to visit. Although it's fetishistic logic was apparent most during such an event, "subject preparation" was a standardized aesthetically driven practice of the ASI archaeological excavation. A trench, a stratigraphical section, a structure, or an artifact, after its excavation and discovery underwent a process of "subject preparation" – cleaning, labeling, and preparation for display, drawing, or photography.

Once it was deemed that the object was ready, it was prepared so that it could be documented through a range of representation media. 'Subject preparation' was the first act that marked the end of the excavation practice and the beginning of the documentation process. It was an essential representative practice in ASI archaeology through which the material culture discovered was transformed into a neat, showcasable evidence about the past. This was the theory of practice through which an excavated object, quadrant, or trench, or even the whole site was transformed from excavated artifact into an evidential artifact by erasing the messiness and the murkiness of the archaeological process and giving the archaeological artifact "finishing touches." This consisted of a series of processes and practices that did not involve excavation but consisted of fetishized acts of incessantly cleaning the trench floor, the excavated structures, the exposed artifacts, and the section wall, or of cutting and re-cutting the section wall with an emphasis on keeping the angle between the floor of the trench and the section wall at ninety degrees. At all sites, there were, among the laborers, some who were considered to be "subject experts" – they could cut "good sections," and had the skill to clean small niches in the trench in a "beautiful and proper way" [*sunder aur sahi*]. Here, it was always cosmetic aesthetics that drove the logic of "subject preparation"- the goal of this effort was to display archaeology as a "neat and clean" scientific enterprise. In the daily practice of archaeological science such a fixation on cosmetic performance was a norm. As a photographer at Baror, on being questioned about the essentiality of subject preparation, remarked: "Look, in fashion photography, a woman has to have her complete make-up on before she can be shot. In the same way, in archaeological photography, the subject has to be prepared, cleaned up, and given a 'make-up' before the photograph can be taken."

'Subject preparation,' was largely a performative representative practice, the necessity for which had also been underscored by Wheeler, and followed by the ASI archaeologists as a dictum:

> No amount of mechanical skill is a substitute for the careful preparation of the subject. Clean, sharp angles between the divergent planes of a section, carefully and emphatically cut with trowel, knife, or edging-tool, are essential if the section is to tell its story with the minimum confusion, Furthermore, a spotlessly clean trench is no mere 'eye-wash', if only because it gives the spectator a justifiable trust in the orderliness and accuracy of the work. Even the top edges of a trench should be neatly trimmed and the grass cut and swept along them; a stray blade of grass in the foreground of the picture may be overlooked by the eye but may loom embarrassingly in the lens (Wheeler 1954: 200)

Here, Wheeler emphasized the spectatorial effect of the excavation site and its performative impact of the archaeological trench as a scientific representation zone. This representative performance was effected through the fetishized creation of a "spotlessly clean trench". This, according to Wheeler, was indispensable to creating a representative narrative of the trench which had no trace of epistemological murk. Here Wheeler makes a causality-based link between the tidy physicality of the trench and the orderly archaeological narrative that the trench corresponds to. For the ASI archaeologists and Wheeler, the archaeological excavation process did not end with the excavation of the trench; it was its aesthetic performance that was crucial to the closure of the excavation process. The emphasis was on negating the disjuncture associated with the archaeological project and to prepare its subject within an orderly matrix. In this, the process of creating knowledge about the past was not so important as the performance of the excavation process. This performative and spectatorial subtext was essential not only for the process of documentation but also for displaying the archaeological site as a scientific representative site when visitors like those mentioned above come to the site. The archaeological site, through the fetishized practice of subject preparation, was transformed into a site with a scientific aura for tourist consumption. Time and again, I observed that subject preparation was given much more importance than the actual process of retrieval of data through the archaeological excavation. In the process, the archaeological excavation site was both a location for the production of archaeological knowledge and the performance of archaeology as a scientific act.

Photography

"Excavation is all about photography. We dig because we can shoot it. In archaeology, everything is finally destroyed; all that is left are the photographs that we take. The only real proof that remains of an excavation is the photographs. So ours is a great responsibility," explained the Senior Photographer at the site of Bhirrana - a placid looking, soft-spoken man in his early fifties, who had been working as a photographer in the ASI for more than two and half decades. "I began working in the ASI as my father's assistant. As soon as I finished my metric exam, I started going to excavation sites with him, working with him, helping him." We were sitting on top of a tower – a wooden-iron contraption nearly five meters tall, at the corner of the excavation site, used for taking pictures from a height. Each ASI excavation site had some such wooden tower, which was considered to be an essential piece of equipment in any ASI archaeological excavation. Some Excavation. Branches. had their own "collapsible" tower; others would construct it at the excavation site using locally available wood and bamboo, employing local carpenters. These were used to take images of the trench in order to get a "bird's eye perspective" – a standard visual trope that was customarily employed in archaeological photography.

"Although I did a year long diploma in photography, it was really my father who taught me everything – from preparing a subject in the trench to developing and printing negatives in the darkroom. The diploma was just in order to get the job in the ASI, because the SA at that time told my father that some degree in photography was needed," the photographer explained, as we tried to balance ourselves between two tripods on which a medium format camera and a 35 mm SLR camera were positioned to take pictures of the trenches below. He further continued in a slightly dejected tone, "Now things are very different. I am trying to get my son into the ASI, but it is very difficult. He is a very good photographer. He can work with everything from a Mamiya to a digital camera. But it has been impossible for him to get into the ASI. You see, the problem is not about skill anymore. No one cares what kind of a photographer you are. It is all about money. I will have to bribe the SA to get my son into the ASI as a photographer. In the past two years, there were two posts but he did not get any of them. These posts were sold to the highest bidder. And I am a poor middle class man, sir, we neither have any ancestral property nor do I indulge in any illegal work or outside work. You tell me how I can afford Rs. 2-3 lakhs to get my son a job? Now, he has to be content making

wedding and party photos. I have also opened a small shop for him; he has become a businessman. The ASI is not in his destiny [*kismet*]," he unhappily noted. Like most conversations I had with many informants, this conversation also veered from a discussion about the technicalities of archaeological practice to various personal and public existential worries that the ASI employees suffered from in their daily existence. We were sitting on top of the wooden tower next to the newly excavated trenches, which were to be photographed. They were four mature Harappan trenches, which were opened up to expose part of the fortification wall of the habitation. The Wheelerian balks had been removed and an area of twenty by twenty meters had been prepared for photography. This process had started a couple of days earlier when the site director had ordered that the excavation in the last quadrant be stopped and the trenches be prepared for photography.

Nearly forty laborers had been assembled to do this large "subject preparation" – a process I was told would take "one full day, and only in the evening, when the harsh sunlight has gone, and the features of the trench are explicitly visible that we will take the photograph," the AA told me early in morning when I decided to spend the whole day with the photography crew. "The Nagpur Excavation branch is not only the oldest, but also technically the best Excavation Branch," he noted with subdued pride. "You have to see how the senior photographer takes picture. This is the perfect way to do archaeological photography. This is the best it gets in the ASI," he further announced decisively.

Since morning, I had been trailing along with the senior photographer and a group of laborers carrying the photography equipment along with the photographer's assistant, who was "actually a fourth class office peon, but he has always assisted me. He is better than most photographers in the ASI. He does not have a degree or formal training, so he cannot become an ASI photographer. But you tell me, does one need a degree to do photography? It is all about experience, but how will the government understand that? They only know how to follow rules," noted the senior photographer. Early in the morning, during the muster roll call, the senior AA relived laborers working in four quadrants in the northern part of the excavation site. He informed the trench team leaders: "today we have to prepare the subject. So there will not be any digging. You and your mates will only take orders from the Photographer sahib." And soon, around forty laborers, both men and women, picked up the tools - brushes of different types, scrapers, trowels, and went along with the photographer and his assistant

towards the trench that had to be prepared. The balks of the trench had been demolished a few days ago, and cleared of the dirt. By mid-day, most of the site was cleared, and each and every excavated structure in the trench looked "spotless." During this process, the workers first used large brushes to clean the area. After lunch, it was "time for the small brushes, to make the subject spic and span" Now the laborers used more fine brushes and cleaned up dirt from the crevices of small objects and structures in the trenches. In the meanwhile, some laborers, who were expert "section cutters", spent time perfecting the section, removing bulges and depressions, using multiple shapes and sizes of scrapers, especially made for the purpose by the local iron-smith. With the help of a plum-bob and spirit level, they worked to make the section wall perfectly perpendicular to the trench wall using considerable amount of time and energy.

At the same time, the photographer and his assistant, with a knife in hand, began a coordinated series of activities. The photographer, perched on his tower, looked through the viewfinder of the camera and instructed his assistant to tell the laborers to clean up areas that he thought looked unprepared or messy. In turn, the assistant would direct the laborers to the particular spot that needed attention. The photographer also instructed the assistant to use his knife to deepen the marks on the floor or those of the stratigraphical layers on the section walls, or the bricks of the mud brick fortification. His assistant would make those marks that were not easily visible through the camera viewfinder, deeper and noticeable. On my questioning about the necessity of this process, the photographer explained, "We are not making any new marks. The archaeologist has made them, and we obviously cannot tamper with that; we just make them more visible. It is a different thing when you take a photograph. If one cannot see the marks in the viewfinder, you have to make them deeper. After all, this is what will remain – so we have to make sure that the markings made by the archaeologists are seen and obvious in the photograph. If it cannot be seen in the photograph, then the Director will get upset," he explained, slightly irritated with my slowness because he thought that his reasoning was "commonsensical" [*commonsense ki baath*]. This practice was not unusual and was followed throughout the archaeological community in India. Wheeler also emphasizes the need to cosmetically enhance the look of the trench for photographic representation: "strata readily distinguishable in nature may merge in the black-and-white of the plate and may, on occasion, have to be emphasized by careful spraying or by additional smoothening or even deliberate roughening" (Wheeler 1954: 2001).

Most ASI photographers were not trained as archaeologists but as photographers and worked under the direct "orders" of the archaeologist officers. They were, as an AA very categorically stated, "technical staff - they are supposed to do what we tell them." The day to day working of an archaeological photographer was to produce photographic representations that were dictated by "Wheelerian thoughts". Although Wheelerian ideas of archaeological photography were based on numerous manuals of archaeological photography available during his time (for instance Wheeler cites Cookson 1954 in Archaeology from the Earth), all the photographers I spoke to informed me that they had learnt about the specificities of archeological photography from seniors who had learnt from: "their seniors who had worked at the time of Wheeler." They were trained as photographers but it was only through extended training under a senior ASI photographer that they had learnt the technique of archaeological photography.

This method of archaeological photography, like most archaeological practices in the ASI, as I have shown in the preceding chapters, had a clear Wheelerian genealogy. For Wheeler, archaeological photography was an essential process through which evidential data that was created during an archaeological excavation was transformed into archaeological knowledge. In the beginning of his chapter on photography in Archaeology for the Earth, he notes, "the overriding difficulty of an archaeological photographer is to induce his camera to tell the truth" (Wheeler 1954: 200). The job of the photographer in the ASI was to produce images through which the "truth" of archaeology could be captured and reproduced. The skill of the photographer was exhibited in the degree to which his/her photographic representation had the ability to produce the truth, as Wheeler noted: "The quality is as much a matter of proper emphasis of accumulative statement, and not a little of the photographer's time and skill, both in the field and in the studio, are devoted to the rescue of the more from the less significant" (Wheeler 1954: 200). For Wheeler, like the ASI photographers I worked with at all the sites, the act of taking photographs was a process through which truth had to be represented as truthfully as possible. This was "commonsensically" situated within a structured convention of emphasizing the truth, which negated the messiness of an archaeological excavation. Representative truth in an ASI archaeological photograph was a performative truth that not only involved a cosmetic operation of preparing the subject but also involved the usage of an important epistemic artifact – the scale.

As the photographer was looking through the viewfinder directing the assistant about what steps to take, he simultaneously removed the labels marking the stratigraphical layers and substituted them with labels that were especially used by the photography team. These were much larger than the ones used by the archaeologists; and made of thick cardboard with the number etched in very black ink. With the help of a laborer he pinned the labels on the layers in alignment with the floor - a black and white meter scale was placed at a very specific location by the assistant according to the instructions of the photographer, as he shouted commands peering through the view finder of the camera. This was the epistemic marker that played an important role in transforming archaeological evidence into archaeological knowledge, as it framed the archaeological evidence within the rubric of a scientific representation system. The practice, I was explained later by the photographer, was to make sure that the "scale has to be parallel to the plane of the camera, or it looks very bad and out of place."

I observed that in each and every photograph that was taken by the ASI photographer on all the sites that I worked – the scale was crucial paraphernalia. I was time and again told by my informants that the "biggest difference between archaeological photography and any other photography is the scale. This makes the archaeological photograph scientific. If the scale is missing, then it is like any other photograph. Both the labels and the scale are key elements of archaeological photography." The use of the scale as an epistemic marker in archaeological photographs is a common means of transforming an arbitrary sign of the past into scientific knowledge that inscribes an epistemic certainty which cannot be challenged. The role of the scale is even more crucial in an archaeological excavation, as it is a destructive means of knowledge production that can never be challenged or tested again at a particular trench or location. In this process, the photographic document with an epistemic marker transforms the moment of discovery into empirical evidence and inscribes on it a concreteness which may not be questioned. Thus, the scale becomes the most important signifier of an archaeological photograph. For Wheeler, the scale is an essential part of an archaeological photograph, and its relevance is so rational that he notes "every archaeological photograph should include a scale, either in the form of a graduated rule or rod or that of a human figure." He further underscores the importance of the scale in archaeological photographs: "The scale should normally be parallel with the plane of the camera-plate; if the latter is tilted, the graduated scale should be correspondingly tilted, otherwise the graduations are in perspective and of variable length.'

(Wheeler, 1954: 201). The centrality of the scale and its importance in the transformation of an arbitrary subject is so overwhelming that Wheeler is forced to add a note of caution: "On the other hand, the scale should not monopolize the attention of the spectator. A central scale, is for this reason, usually bad" (Wheeler, 1954: 202).

Much before the senior photographer from the DG office came to Dholavira, I was more than once made aware about his technique of taking archaeological photos: "he is the best photographer in the ASI at the moment. He has won many awards. At whichever site he has worked, it has been his photographs that are published in the final report," informed a junior photographer at the site. "He is so busy that he does not stay at one site for more than a week. He is always touring. He has even gone abroad on ASI missions," continued the junior photographer who I had been following and working with since I came to Dholavira. So when the chief photographer came to the site, I decided to religiously follow him and observe him as he took photographs during his stay. In a characteristic ASI style, he conducted his work with a series of assistants; a peon and a group of laborers carrying tripods, camera bag, scale, black cloth, brushes scrapers, and other photographic paraphernalia. These laborers had been hand picked by the chief photographer, because they were experts in subject preparation, trained specifically by him over the years. Whenever he came to Dholavira to take pictures, this group of three-four laborers was relieved of their work in the trenches so that they could work with the chief photographer. Their work would start early in the morning and the whole day was usually utilized for the preparation of the subject and it was only in the evening in the "angled light" that the subject would be photographed.

During one such photography event, one late afternoon at the massive citadel wall of the Dholavira, the chief photographer with a team of assistants were preparing to take photographs of the whole monumental structure. The Dholavira citadel fortification wall was justifiably called a "monumental structure;" it enclosed the entire citadel mound. Made of rock cut bricks, it had an awesome presence and together with the rock cut reservoir, and the huge eastern reservoir lined with rock cut bricks comprised the monumental architecture that made up the site of Dholavira. A few days ago, it had been decided by the co-director that it was essential to take a "perfect picture" of this citadel fortification wall. But he had been waiting for the chief photographer from Delhi to come, about whom he had also spoken very highly. Standing on top of a soaring collapsible aluminum tower, especially bought from Ahmedabad

and transported to Dholavira in a truck a few years ago, the chief photographer was shouting orders in Hindi and a laborer assistant was translating them into Kutchi Gujarati and instructing around two dozen laborers who were cleaning and preparing the monumental subject. The sight was spectacular - the chief photographer, a slightly bulky man, with a white cricket cap, jeans, sneakers, and a photographer's jacket, was precariously balancing on top of the fifteen meter tall collapsible tower, which had been fully stretched. Strapped across his chest were three 35 mm SLR cameras with a back & white roll, a color roll, and a transparency roll respectively, all of them fitted with zoom lenses. On the ground, were nearly thirty male laborers, trying to keep the aluminum tower, bent under the weight of the chief photographer, from swaying by tightly holding on to three thick ropes that were keeping the tower from crashing.

Since the subject – the citadel fortification wall – was so enormous, instead of the black and white meter scale in an archetypal ASI representation style, the human scale was to be utilized. After the subject had been cleaned and prepared, the chief photographer instructed his assistant from atop his fifteen-meter tower: "get me a man in a dhoti and turban [*pagdi*]. I don't want a man in trousers [*pantwala admi nahi chahiye*]. And also get a woman with bangles [*churiwali aurat*]." In a few minutes, the assistant had assembles a couple of women dressed in typical rural Kutchi attire with *ghagra-choli*, with both their arms adorned with white plastic bangles. But there was no man with a dhoti around. When the chief photographer commented on the absence of the man in the attire he had wanted, his assistant informed him that the only man who wore a dhoti on the site "did not come to the site today". It was at this moment that the chief photographer climbed down the aluminum tower and walked toward me. I was standing and observing the whole proceedings from under the shade of a tree. Along with his assistant, the chief photographer came over and requested "sir, will you be our scale? You are the only one on the site wearing a dhoti." At first I flatly refused, but after a little bit of cajoling from the photographer and an AA who was standing beside me, I hesitatingly agreed to be their human scale. For me, it was a moment of epistemic epiphany, for just a couple of years ago, I had written an article critiquing the usage of human scale in the photographic presentation of the ASI (Chadha 2002). And here I was, suddenly standing on the steps of the citadel fortification wall of Dholavira, posing with a brush, looking away from the camera, in my dhoti-kurta - not really looking like the rustic Kutchi laborers I was supposed to represent, but "ethnic" enough to be made part of the ASI photograph of an

archaeological monument. The chief photographer had also requested me to remove my glasses to look "authentic." I posed for nearly an hour as the setting sun cast its long shadows on the site and the chief photographer, on top of his elevated aluminum tower, screamed multiple instructions at me and the two Kutchi women. One of them was given a brush and asked to squat and pose as if she was cleaning the floor of the fortification wall and the other was asked to stand in one corner of the fortification wall with a wicker basket on her head. The photographer instructed them with regard to how to stand, how to hold the brush, how to squat, and even where to look. If the chief photographer was unsatisfied, then one of his assistant laborers would go to the women and physically direct them how to stand, squat, or hold the brush. Once he was fully "satisfied," under the lengthening shadows of the setting sun, atop a fifteen-meter tall collapsible tower, he clicked pictures with each of the three cameras. Below him, thirty laborers kept the chief photographer afloat and another few dozen laborers stared at the elaborate performance, as I, together with the two women from Dholavira, were forever inscribed in the scientific representation of the Dholavira fortification wall.

After the hour-long ordeal, I was released, and, on the way back to the camp, I asked the chief photographer a few questions about the process that had undertaken. He explained, "Whenever I use a human scale, I make it a point to use the local laborers. I try avoiding the laborers dressed in modern western clothes. The more traditional attire the better it is." On asking why it was better, he answered, "because Dholavira is an ancient site. We know little of what the people wore and we surely know that they did not wear pants and shirts, like these men folk today," he disparaging remarked about the western dresses wore by most men in Dholavira. "The closest we can get to the original, is by putting people who are dressed in their traditional attire in the picture. That is why I want woman with bangles. Because we find so many shell bangles in the Dholavira and these woman folk here wear bangles uptil their shoulders. They are like the Harappan dancing girl. That is how you make the photograph look authentic [*asli*]." But when I responded, "isn't the human used just a scale?", he answered "Yes, their primary purpose is that of a scale but they are not inanimate objects - they have a character so we have to exploit that to the fullest."

The usage of human scale was a set practice in archaeological representation and its genealogy is located in the use of human figures in picturesque paintings of ancient monuments in the

colonial landscape in the late eighteenth and early nineteenth centuries. In the Indian context, it was Wheeler who enhanced the human figure into an epistemic artifact essential to transform the representation of an archaeological subject into a scientific artifact (Chadha 2002). Wheeler is eloquent about the necessity of using the human scale and is very particular about its role in an archaeological photograph:

> Where the scale is a human being, as it is often desirable in large subjects, the individual thus honored must remember that he is a mere accessory, just so many feet of bone and muscle...Two axioms of the use of the human scale are (1) that the figure shall not occupy a disproportionately large portion of the picture (if so, a linear scale must be substituted), and (2) that the figure shall not look at the camera but shall be ostensibly employed in as impersonal manner as possible (Wheeler 1954: 2002).

The tradition of fixing a subaltern human marker in diagrammatic and photographic representation of archaeological landscape was widely practiced in Indian archaeology before Wheeler. But it was he who enforced the marker with an epistemological meaning with scientific credence. The subaltern laborers - men and women, were objectified in the representational lexicon of archaeological knowledge by Wheeler, and utilized as ethnic markers to legitimize the colonial undertaking of inscribing on the subalterns their past. On Wheeler's view, they were incapable of discovering their past themselves - it is only through participation in the statist project that they could engage with it. The subaltern subjects were subverted because, in spite of being given a place of pride in the knowledge production process, they were simultaneously appropriated to authenticate the Enlightenment project of civilizing the native. Even now, they are always shown in these photographs as industrious workmen or women, attired in native robes, who experience the past provided to them and are deeply engaged with it while doing menial laborer - cleaning. This representation of the natives by fixing them with work that they did, in their traditional attire, along with their tools of trade, was a marker of typicality that signified their ethnicity (Pinney, 1997: 53).

Pottery Yard and Ceramic Classification

The largest number of artifacts unearthed in any Harappan excavation was of ceramic fragments – potshards of different sizes, texture, color, shapes, and material. For the ASI archaeologists, the ceramics excavated were the key artifactual building blocks through which factual knowledge about the site was constructed. Specifically, in the conventional culture history paradigm, the ceramic evidence unearthed at the site was used to construct the cultural

sequence of the site, subsequently analyzed to relatively date each of the cultural layers. Therefore its proper collection, classification, and documentation were at the center of post-excavation work in any archaeological site. The sheer magnitude of ceramic artifacts in a Harappan site necessitated an elaborate sequence of processes for their organization, compilation, and categorization. From the moment of their discovery, ceramic artifacts had a trajectory disparate from other artifacts excavated at the site, through which they were finally designated as factual evidence for archaeological knowledge production. Other artifacts, distinguished as antiquities in the ASI terminology, did not have such indispensable chronological value. The very fact that artifacts other than ceramics were termed as 'antiquities', emphasized their worth in terms of rarity and value. Although the ceramic artifacts found at the site during excavations were equally "antique" they were not deemed to be valuable because they were discovered in such abundant quantities. However these ceramic artifacts, for this same reason, were viewed as valuable in facilitating the archaeologist to delineate the temporal and cultural sequence of the site. Unlike other artifacts excavated during the course of an excavation, it was only through careful study of the ceramic artifacts that a chronological history of the site could be delineated. It was this framework of engagement that determined the distinctive practice through which the ASI archaeologists "managed", studied, and epistemologically fetishized, or discarded these ceramic artifacts.

Each 'dig' in a quadrant produced a vast number of ceramic artifacts. In most cases, the potshards excavated were shattered fragments, and in rare cases, complete ceramic vessels would be found. To give an idea of the magnitude of ceramic fragments found during each dig, let us look at the different sites. In habitational deposits and living spaces in the houses of the citadel in Dholavira, dated to Mature Harappan period, around hundred to hundred and fifty ceramic fragments of various types and shapes would be discovered in each quadrant. In Bhirrana, where the excavation was primarily being conducted in habitational deposits categorized as Early Harappan, each dig would generate less than a hundred ceramic fragments. In both cases, each dig would be around six inches deep. In a site like Hansi, on the other hand, where the excavators were mainly digging through early medieval and historical layers in quadrants of similar sizes, each dig would be about a foot deep, and the ceramic count would be between two to three hundred. Thus, the ceramic count depended on two factors – the type of cultural deposit and the depth of the dig. In sites like Dholavira, where nearly two-dozen quadrants were excavated simultaneously every excavation season, the total

number of pottery fragments unearthed would be more than a hundred thousand. To manage, sort, classify, categorize, document and then to study them was an arduous and laborious task. During my ethnography, I observed that the daily practice through which the excavation team engaged with this huge volume of archaeological evidence instrumentally, functionally and pragmatically was as a "management issue," rather than a classificatory or an epistemological problem.

The huge amount of ceramic artifacts being unearthed at the excavation site "was both a curse and boon to the process of the archaeological excavation" noted an AA in Dholavira. He explained, "these Harappan sites produce so much potshards that it becomes a management problem. All we need for the site report is some diagnostic pottery - the rest end up in the Purana Qila Godown." Purana Qila was the famed sixteenth century medieval fort in Delhi, now housing within its rampart walls, the Central Antiquity Collection of the ASI. The Central Antiquity Collection (CAC) was a centre for the collection of the explored and excavated pottery and other antiquities of the ASI. The CAC was created in the 1910s to mainly house the explored antiquities from Sir Aurel Stein's Central Asian Expeditions (1906-1916) . Purana Qila in the ASI lore was, as an AA at Dholavira expressively noted, "the final resting place for ASI artifacts." He further elaborated "once an artifact ends up in Purana Qila, it requires an enormous amount of paper work even for senior ASI archaeologists to obtain it even for examination. Purana Qila is the death bed of the ASI artifact." However, I observed that the trajectory of a potshard from the quadrant to the tin trunks in Purana Qila involved numerous intermediary stages. The ASI archaeologists dealt with ceramic artifacts as a "management problem," which necessitated a process by which only some ceramic artifacts ended up in the CAC in Purana Qila. The rest , I observed, were reburied.

It was one hot afternoon in Dholavira when I noticed that behind the mud hut of the Director was a five-meter by meter trench in which an enormous amount of ceramic fragments had been dumped in a careless manner. On enquiring, the AA who was accompanying me explained, "this is useless pottery, we don't need it. At the end of every season, we throw away all the potshards that are useless [*koi kaam ki nahi hai*]." On further prodding, the AA explained, "these are pottery pieces which don't serve any function. You know well how much of pottery is excavated in a season. It is virtually impossible to keep all of it. In the pottery yard, the important fragments are sorted out and the rest are thrown here". This was

called the "pottery-dump" and I observed that it was a normal feature at each ASI site. Here, ceramic fragments that were deemed to lack antiquity status were discarded. In the pottery yard, during the sorting and the classification process – diagnostic potshards were separated and categorized within the ASI theory of "antiquity." These were those ceramic fragments which either belonged to a vessel that could be reconstructed in the pottery shed, or were fragments with distinctive fabric, shape, artwork, and design repertoire on them -, which had "some uniqueness." The rest of the ceramic artifacts that were not deemed to be diagnostic were discarded in the pottery dump. "What can we do? It is not possible to cart every single potshard found at the site and take it to the head-office. We have to throw most of them. Or it will lead to data-overflow. As it is, it takes decades to write site reports, if we have to analyze each and every potshard, the report will never be written," noted another AA, with irritation. He was in charge of the pottery yard in Baror.

The notion of diagnostic ceramic was central to the theoretical structure of culture history archaeology. Diagnostic ceramic consisted of those ceramic fragments or complete vessels that defined the cultural period of the layer or period. The material culture sequence of the site was drawn by their typological characteristic. Stratigraphical layers of each quadrant were culturally delineated by the typological classification of these "diagnostic poetry." Very few of these diagnostic ceramic fragments, which had "unique value", attached to them ended up on the table of the artist of the pottery yard for documentation purpose. The unique value of the potshard was defined by what an ASI artist in Dholavira explained "we only select those pieces of pottery which can give us a clear idea of the unusual make, distinct shape of the period, those that have good designs on them or those that have some sort of inscription on them." So diagnostic ceramic fragments were those that fulfilled the definition of 'antiquity' and those that were deemed valuable.

Comparable to the awesome spectacle of large-scale ASI excavation, was the representative post-excavation spatiality of the pottery yard. The pottery yard was the two-dimensional physical representational structure of the three-dimensional excavation site - a sophisticated classificatory conceptual architecture for an ASI archaeological excavation site. It was an elaborate two-dimensional grid, laid out on the ground, representing the three-dimensional Wheelerian excavation site. Not only the ASI, but every archaeological site in India had one such pottery yard. Here, all the ceramic material culture and animal bones were stored,

categorized according to the quadrants and layers from which they had been unearthed. The pottery yard was usually situated outside the excavation area, adjacent to the campsite. It was an open-air, large gridded two-dimensional structure in which the ceramics fragments and the bones found at the site were neatly arranged in square symbolizing a single quadrant. Each square was neatly made up of lined stone pieces and brickbats, often colored with white lime to announce their official-ness. These lime-smeared pieces of stone architecture on the ground consisted of hundreds of one meter-by-meter squares laid out in a grid. Each trench was represented by four squares corresponding to the four quadrants of the trench. Similar to the Wheelerian balk, groups of four squares, representing trenches were separated by a path through which laborers could walk. The squares were tagged with an iron peg stating both the trench and the quadrant number. In each square of the grid there were numerous heaps of ceramic fragments, each representing one layer of the quadrant. Tucked under the heap would be the label, specifying the quadrant number, the layer number with depth, and the name of the trench supervisor.

One AA would be given the charge of the pottery yard with about two to three technical staff members manning the laborers in the yard. There was clear division of labor in the pottery yard. Every evening, after the excavation ended, each trench supervisor would come to the pottery yard. He would be accompanied by laborers carrying all the ceramic and animal bones found on that particular day in wicket baskets and the latter would neatly put the contents of these baskets in the square representing the quadrant. Subsequently, each ceramic fragment would be cleaned and labeled before sorting and classification. The task of cleaning the ceramic fragments and then labeling them was continuous as the flow of potshards never stopped at the site. Everyday, in one corner of the pottery yard, a group of women in their colorful printed attire covering their whole body and their head, with their face barely visible, would be squatting and scrubbing ceramic fragments with water and toothbrushes and then drying them in the open sun. An AA told me that the women had been specially trained to scrub the potshards gently so as to not erase the pattern or design on its surface. Once dried, one of women would take the potshards batch by batch and cart them to pottery shed. Here, usually very young boys, sitting cross-legged, would be labeling each ceramic fragment. They would painstakingly write with black or white indelible ink on the inner surface of the each fragment the abbreviated name of the site, the trench number, the quadrant number, and the year of the excavation season. Among the laborers, both these tasks in the pottery yard had a

very significant meaning; work in the pottery yard was considered to be very comfortable [*aaram ka kaam*] as it involved very little labor and movement, unlike other tasks at the excavation site. I observed that the task of cleaning was usually done by elderly women laborers at the site, who were too frail to work in the trenches. The task of writing and labeling potshards was only awarded to the educated laborers at the site, as it involved at least minimum proficiency in English – to be able to read the labels and then copy the information onto the ceramic fragment. Most of these were young boys who had studied till grade eight or ten in school.

The AA who was in charge of the pottery yard in Bhirrana was not a ceramic specialist but considered himself to be "interested in pottery." Describing the work he did at the pottery yard, he told me: "I have been working out here in the pottery yard since the excavation started. No special reason, I just wanted to learn how to work with pottery. So the site Director told me to take care of the pottery yard. It is not that I don't work in the site. But my primary task is to organize the pottery found in a systematic way." In another conversation, while I was observing him and the laborers in the pottery yard, he pointed out to me: "specialists are in a short supply in the ASI. You have be "jack of all trades, master of none" in the ASI. An archaeologist in the ASI has to be everything. First and foremost he is a bureaucrat. Then he is an archaeologist. And as an archaeologist, he has to learn to be a pottery specialist, bone specialist, excavation specialist, everything. That is an ASI archaeologist. To tell you the truth, they don't encourage specialization in the ASI," the AA noted sadly. There were very few specialists in the ASI, as I have noted above - there were no archaeo-zoologists, archaeo-botanists, archaeo-metallurgists nor were there any lithic or ceramic specialists. These experts were usually invited from various university departments and asked to study the materials and invited to contribute in the final excavation report when it was written. At Dholavira, I was told that a number of such specialists had been invited at the discretion of the site Director. An AA at Dholavira said, "over the course of the years, numerous specialists have visited the site, worked for few days, and then vanished. Some even came from American universities, but we have not seen anything written by them, Probably we will never. They will only be published when the Dholavira report comes out." Since such university specialists were in short supply, the work requiring such specialization at the site – especially pertaining to pottery and bones – was done by recent PhD students. These were students who had just finished their PhDs and wanted the experience of working in an ASI excavation site or were unemployed looking for

interim work. They were paid a fixed salary and hired on a monthly basis, and in the site hierarchy were considered at par with the ex-Institute students. The usage of such specialists who were recent PhD students was far more common than inviting University professors as specialists, and it reflected not only the desperateness of the students working but also the intellectually impoverished state of the ASI.

At Baror, I observed one such specialist archaeologist. who had recently finished his PhD and was working on the ceramic assemblage of the site. He was not an employee of the ASI, but had been asked by the site Director to work and study the pottery at the site since his PhD was focused on ceramics. We were sitting adjoining the corner of the pottery yard, in colorful molded plastic chairs, shaded by a rainbow colored field umbrella that was just big enough to provide shade to the two of us. Explaining his status at the site, he told me: "I am just a glorified daily wager. They pay me a fixed amount, as much as the ex-institute students, along with a tent to stay in. There is no value for my PhD here; they just wanted some one to take care of the pottery so I am here. I did not have a job and I wanted some excavation experience so I am here." On the white plastic molded table between both of us was a cluster of pottery fragments, and two laborers were standing and counting the pottery fragments. A third laborer was going back and forth between the pottery yard and the table where we were sitting, ferrying pottery shards. The ceramic specialist was sitting at the table with a chart called the "Frequency chart of the Pottery." This was a chart with 32 columns divided into five clusters. The first cluster contained columns for trench number, quadrant number, layer number, and depth; the second described the type of fragment: rim, belly, base; the third cluster had columns for the type of pottery; bi-chrome, rusticated, deluxe, incised, buff ware, gray ware, red ware, Hakra ware, perforated, cream slip ware, chocolate slip ware, black & red ware. The fourth group of columns were related to the shape: lota, vase, basin, bowl, disc, storage jar, trough, *handi*, miniature, goblet, flowerpot, and tumbler. The laborers counted the number of pottery fragment in each heap – representing a layer in a particular quadrant. The ceramic specialist noted the number of fragments and their traits and characteristics in the chart that he had spread on the table. I was told by the specialist that such ceramic analysis was standard in the ASI. After the notation of the entire ceramic fragment, only the "diagnostic" pottery was collected and separated from the rest, which were eventually discarded. The fragments retained were set aside because of their distinct shape, size, or design; most of the discarded fragments were considered "too small, too useless [*bahut chota, bahut bekar*] for any

analysis." The decision was primarily guided by an antiquated cultural history logic of archaeology, centered on just the obvious and visible cultural traits of the material culture, which were important to a research approach that focused on cataloging, descriptive study, and database development.

Antiquity Class

Every evening, after the excavation, the students and excavators would trudge back to their tents in the campsite to take hot water baths and "get fresh." After this they would all congregate in the antiquity class to submit the antiquities that had been discovered in their trenches. The "antiquity class" was a regular post-excavational ritual at the ASI excavation site and attracted all the student excavators, trench supervisors, AAs, and some technical staff. The antiquity class was usually held in a special tent, which contained a number of padlocked tin trucks containing "significant" artifacts that were discovered over the season. The class started around two hours after the excavation and went on until dinner time. Its primary function was bureaucratic - it was during the antiquity class that the excavators officially submitted their excavated artifacts to the AA in charge. These artifacts were recorded in the trench notebook – with measurements, provenance, and depth; it was also during this evening ritual that the artifacts were handed over publicly to the AA who would safely keep them in the antiquity trunk and lock it.

Antiquities, as I have explained earlier in the chapter, were treated differently from artifacts by the ASI and were at the centre of the antiquity class. It was during this meeting that various artifacts were passed around for other excavators to see, observe, and feel. Discussions were held about the nature and purpose of objects whose functions could not be definitely ascertained. These discussions led to a consensual opinion about objects whose material or purpose were doubtful, so that this information could be documented. Often, during this process, objects were re-labeled. Usually, each antiquity was registered and recorded in a number of archives: first it was labeled and kept in a separated plastic or paper container, then recorded in the trench notebook by the trench supervisor and then subsequently, during the antiquity class, it was given to the AA in charge of the site's antiquities, who recorded it in the antiquity records notebook. The epistemological significance of the antiquity class was that it was during this meeting that artifacts designated as antiquities by the excavators were

transformed into a knowledge paradigm. Labeling artifacts in the trench constituted the first step in transforming archaeological evidence into knowledge; however it was in the antiquity class that an artifact was firmly assigned its position in the primary representational texts – the site notebook and the antiquity record book. The Director of the excavation later referred to these knowledge artifacts in the process of writing the final authoritative representational text – the site report.

The antiquity class was not a feature of every ASI excavation camp, but it was a ritualized event in those excavation sites where field schools were held. An AA, who regularly led the antiquity class in Dholavira explained, "This is an old custom. People say that it is as old as the Taxila field school. But I doubt it. However, most ASI sites have it. Especially those in which students from the Institute come for field training. At Dholavira, we only have it when students come. If the students are not there it is not held." Not just at Dholavira, but at all the sites I worked in, the antiquity class was regularly held only when the students of the Institute were present. It would be discontinued when the students left. The antiquity class was primarily a pedagogic ritual where the senior archaeologists at the site gave lectures about the "nitty-gritties [*dau-pach*] of the excavation method." The importance of this class was underscored to the students who attended excavations for the first time and they were warned that the antiquity class required "compulsory attendance." Lectures on stratigraphy, excavation methods, typologies of Harappan ceramics, the various divisions of Harappan phases were explained during this period. As a student at Dholavira noted, the antiquity class was "more about antiquity than a real pedagogic class. The lectures that are given are nothing new. You can learn all about that in the books. But it is only useful because we are able to talk about discoveries in other people's trenches, and see and feel the antiquities that have been discovered."

While the overt goal of the Antiquity class was pedagogical, it was also a practice of socialization into the nuances of the archaeological method. During this class, students shared their material experiences of the excavation and were socialized by senior ASI archaeologists into the technical nuances of the excavation process. The most important element of the class was the process through which students and archaeologists would arrive at a consensus regarding the functional nature and material of objects which were unidentified or unknown. The antiquity class was analogous to the pottery yard where the antiquities would be situated

within an epistemological framework. However, the crucial difference was that the antiquities did not undergo the rigorous categorization process that the ceramic artifacts discovered at the site were subjected to. This was because in the culture history method that the ASI followed, and antiquity did not have any chronological significance but only had a value attached to it.

After a very exciting antiquity class during which the students were able to feel and touch "first hand" a Harappan seal that had been discovered in a trench in middle town in Dholavira, the AA very sadly noted "this is the only time you will ever be able to see this artifact." The Harappan seal was the rarest of any artifact found at the site; it was considered to be the most "valuable antiquity" of Harappan excavation. "Only a couple are found each season," noted the AA, "if we are lucky, at most five - anything more than that is a miracle." So to actually see, feel, and touch a Harappan seal was considered by many to be the highlight of an excavation season. But the AA remarked "the apathy of the ASI system is that after an excavation, an antiquity like a Harappan seal disappears into the tin trunks of Purna Qila. God knows how many seals are lost there. No one knows. This is the last time you will see this seal. If the seal is lucky, it might be published, and if it is very lucky, it will end up in the National Museum. But it is more or less out of reach for researchers because it is a very valuable antiquity and the ASI bureaucrats will not let anyone get any access to it." In this context, B.B Lal, in the preface of the Kalibangan report, notes with regret the difficulty in obtaining Harappan seals from the excavation that he was a co-Director of. He remarks:

> In 1972, the writer took voluntary retirement from the post of the Director general of the Survey and joined Jiwaji University, Gwalior, as Professor and head of the School of Ancient Indian History, Culture and Archaeology. During the summer vacation at the university in 1973, he came to Delhi with the specific objective of writing the report on the excavations at Kalibangan. He wanted to begin the work by analyzing the data regarding the seals. The idea was to find out if seals with a given motif occurred in any particular context or not. And if they did, what could be its implications. He had already made some eye-copies of the seals during fieldwork. All that he wanted was to examine the plans and sections of the trenches wherein the seals concerned had been found. He requested the authorities to make the relevant drawings available to him. He waited for more than a month, sweating in the sultry room at the Survey's Safdarjung office, but nothing was done. Excuses of one kind or another were produced. Completely disappointed, he had to go back to Gwalior, informing the

authorities of the situation and adding that as and when the records were made available, he would come back and resume the work. That day never came and the report-writing was thus at a standstill for quite some time (Lal 2003: v)

This highly public disapproval of the ASI by an ex-DG of the ASI reveals the inadequacies of the process through which antiquities excavated at the site disappear into the unknown crevices of postcolonial governmental archives. In this context, the ritual of the antiquity class at the excavation site where the excavators have an opportunity to materially interact with archaeological artifacts becomes fetishized as the site of both an epistemological and an ontological engagement with archaeological artifacts.

Research Design

"The research questions for the SHP have already been decided by the bosses in the DG office," acerbically noted an ASA in Baror, "We come here to just dig holes. There are smaller day-to-day research questions – such as which trenches to open, which quadrants to dig, and so on but they are minor questions [*choti baathye*]. At the end of the season, we have to see that all the money that has been allocated to us for the excavation has been spent, and that we have enough data for the Annual Report. That is our primary aim," sardonically continued the ASA, who was very disenchanted with the ASI excavation process, after having been in the ASI for more than a decade and a half. However, when I spoke to the site Director at Baror, he was more precise and explained to me the details of the research design in terms of the need to excavate the early Harappan layers and demarcate the extent of the habitation deposit, to trace the fortification wall, to get the complete sequence of the culture history of the site, to expose at least one complete house structure. His narrative about the SHP research design was the "official answer," according to an AA who was also sitting with me while I was talking to the site director. He then continued "but as you can yourself see, the excavation is in a mess. They are just digging arbitrarily. In two seasons there have been three site directors - the first was an absentee site Director, who came here only for the inauguration of the excavation, as she also had joint assignment as the SA of a Circle. Then there was the onsite director who would only sit in his tent. He only wanted to ensure that the money allocated to the excavation was finished by the end of the season, and that he could pocket some in the process. And now there is a new site Director, who is the present SA of the Ex. Br. He has been here just few weeks, and this is the first time he is digging a Harappan site. He is a Buddhist specialist from Eastern India. He cannot make out the difference between red slipware and black and red

ware. He does not have any clue about what is going on here. We know what is happening here. It is PWD (Public Welfare and Development) work. Dig because there is a site. The only research questions involve day-to-day problem solving and management problems. That's all."

At Dholavira, when asked about the research design, a veteran AA told me: "It has been the same for the past ten seasons. The important thing is just to dig. Now that Bisht sir is busy in the DG office, he does not care about the day-to-day workings at the site. The report is nowhere in sight. All that we are told to do is to dig. Find more reservoirs. Dig the Middle town. Dig the citadel. You tell me how much more can you find after having worked for more than 12 years. I think this is just glorified treasure hunting. Dig antiquities from the earth and then lock them up in trunks in Purana Qilla. Nothing more." Questions about research design and research plans would almost always elicit such dismissive replies from informants who could confide in me. Others would give me the standard official reply - the need to search for Saraswati at a macro-level and the necessity of defining the cultural sequence at the micro-level of the site. For most of the junior level officers and the Staff, the research question was dictated by day-to-day management of the archaeological excavation – deciding where the trenches should be opened up or how deep the trenches should be dug. These micro research questions were considered to be administrative issues rather than academic queries with scholarly implications.

Archaeological excavation was considered "routine work" by all the members of the ASI staff that I interacted with. It was "carried out" as a job that had to be done by the employees of the ASI - some members were enthusiastic, others unconcerned. Whenever I asked questions about the research program or the research focus of the excavation project - I got very different answers; sometimes even contradictory. For the non-archaeological staff members, the excavation project was viewed as one of the customary duties in their job profile, as most of these informants saw their job in the ASI as a "government job" [*sarkari nuakari*] which was "permanent and without problems." They believed they had certain skills which had gotten them the job and that the theoretical framework of the archaeological excavation was the job of the archaeologists. "Why are you asking us these questions? We just do what we are told. We go where we are ordered. Look, we know this is a field-based job so we knew before we got into ASI that we have to leave our family behind. That is our biggest problem - being

in the field for months on end, not seeing our loved ones," replied a disinterested draughtsman to me one afternoon, as I was standing with him under the shade of his giant umbrella, as he worked on the drawing board. "Sir, most of us are not concerned at all about research and all that - you should question the archaeologists," he finally responded after my persistent questioning about what he thought the research agenda of the excavations he participated in was.

The technical staff viewed the archaeologists as a group of unsure scientists who had no clue as to what they were doing. Although no one explicitly told me that, it was obvious to me from casual conversations that ASI archaeologist officers were looked upon as confused scholars, who were unsure of the research they were conducting. One day at night, after dinner, I was sitting with a technical staff member of the Dholavira excavation camp. A day earlier, a photographer had complained to me that I was spending too much time with the sahibs – "spend some time with us too" [*hamare saath bhi thoda bahut waqat beetaiye*]. That night I was with them until midnight – the conversation at one point turned towards the archaeologists, and I was told a joke about an archaeologist. This joke captured in a succinct way the difference between the technical staff and the archaeologists in the ASI. "Once I was watching the rerun of the Ramayana serial on the TV with my wife," narrated the photographer who was at the center of the evening's gathering as he was relating jokes [*chutkule*] one after another. "It was the scene in the gurukul in the jungle, the rishis were training Ramji and other heavenly figures [*devata*] in the fine art of archery. The guru asked all the devata to take aim at a mango on a tree far away. But before they aimed, they were asked to tell the guru how far the target was. First it was the turn of one devata, he noted, that 'it was approximately [*shayad*] 40 meters away;' the next devata said that the target was 'shayad 50 meters away;' then another said that the mango was 'shayad 35 meters away.' Then it was the turn of Bhagawan Ram, and he gravely peered at the mango and calmly informed the guruji that the target was 'exactly 43.987 meters way.' 'very good!' [*shabash*] the guru praised Ramji and and let him take the aim. It was then my wife asked me a question- 'so because Ramji gave the right answer, he became a God [*bhagwan*] from a devata. So what happened to the rest of the devatas?' I replied, 'They all became archaeologists.'" This provoked a lot of laughter, and then he explained to me, "See, that is the difference between archaeologists and normal people. They are always about approximation [*shayad ye shayad voh*] - they will never tell you anything straight [*kabhi sahi utar nahi detae*].' As I noted

above, it was through these fleeting comments that I gauged the perception of lower level ASI functionaries about the scholarly abilities of their superiors. It became obvious to me several times that an ASI archaeological excavation was “everything else but research.” As a disgruntled ASA once remarked frustratingly: “It is about money, corruption, public performance, politics, favoritism, personal gains, and everything else. Research is just an excuse. Go and ask any archaeologist and ask what is research and you will know that it means nothing to him.” And this is what I decided to do - to ask various members of the archaeological team what their research program or scholarly motive was in participating in such a large-scale archaeological excavation.

Between the decision to choose to excavate an archaeological site and the moment when the actual excavation begins, is a long and arduous process even for an ASI archaeologist. "We will not even discuss the process through which a non-ASI archaeologist gets permission to excavate. That is a different story", explained a senior AA as we were sitting on top of the thick stone fortification wall of Dholavira, along with two other AAs. It was late afternoon, and we had had just finished the inspection rounds of excavation work in almost two-dozen trenches supervised by Institute students and ex-students. There were still a couple of hours more before the day's work would end. We had found ourselves a strategic position - panoptical to say the least. We could see the work going on in the Middle Town, Lower Town, and the Citadel. The students and ex-student supervisors could be identified by the white baseball caps all of them were wearing to protect themselves from the harsh sun. The laborers were seen crouching on the floor of the trench scraping or digging, others were seen carrying dirt over their head as they walked a few meters to dispose it off in a corner of the mound. "To begin with, only an ASI archaeologist of the rank of DySA can apply for the license to excavate. So we as AA, even if we have PhDs from a reputed University, cannot write a proposal to excavate even a small site on our own. We have to work under some SA or a DySA", continued the AA. This remark also revealed the disenchantment of the AA working at the site under a senior officer. Numerous AAs were unhappily working under officers that they thought were incompetent, dishonest, or lacked a "scholarly mentality." The idea of "scholarly mentality" was important for many junior officers and other staff members. It is on the basis of this professional perception that the junior ASI staff distinguished between seniors they respected or had contempt for in their conversations. I learnt that the characteristic "scholarly mentality" involved not just the ability to excavate a site properly, or to have a

research plan or design but it was the larger make up of an individual officer. As the senior AA explained "scholarly mentality is not about digging holes in a stratigraphic manner, but it is the larger world view of an excavator. In the ASI it has to do with honesty. The honesty to not only dig a site properly and but also to be non-corrupt."

Perched atop Dholavira's mound, I tried to understand what the AA meant, and after a little prodding he explained, although he thought I was a bit dumb to not comprehend what he said: "I think there is clear link between good scholars and non-corrupt scholars. In my experience of my involvement with ASI for nearly a dozen years, I have seen - and this is just my theory. First, according to me, a good scholars [*badiya scholar*] is one who decides to dig sites with some research question(s) in mind, who excavates a site thoughtfully and does not rip open the whole site [*poori site ko nahi uqharte hai*] and those who make attempts to write the report as soon as they can. He is also a good taskmaster. He also is not corrupt. Now it is difficult to say, as you know, who is corrupt and who is not. But let us say, they don't have CBI cases registered against them. And according to my theory - a good scholar is most likely a non-corrupt person." In this description of the scholarly mentality of an ASI archaeologist, the informant combined the qualities that he believed epitomized an academic-archaeologist and conflated them with those of an administrator-archaeologist. It is important to note in the above description by the AA that unlike an academic archaeologist, the ASI archaeologist was also imagined as an unblemished administrator. The idea of 'scholarly mentality' was also tied to other social characteristics, and was not only about astute academic and intellectual capability. And it is this combined quality of an academic-archaeologist and an administrator-archaeologist that made up an ASI officer and differentiated him/her from normal university trained archaeologists. And it is between this tension of an administrator and the academic that the career biographies of senior archaeologists were narrated. Most of my informants noted that there were less than a handful of ASI archaeologists with 'scholarly mentality': "there are not many such people. They were all the old timers. [*ab yase log kahne, woh sab purna jamane ke log the*] Nowadays, you cannot become the Director at the ASI without having a CBI case against you", a DySA noted sarcastically. Almost all the archaeologists that I spoke to referred to Wheeler as the most perfect scholar administrator, and some student archaeologists recalled that their teachers at the institute would often refer to Wheeler as not only the most astute archaeologist that the ASI had ever seen, but also the most efficient administrator. The students were often told that they should aspire to be like "Sir Wheeler".

Before I end this section I would like to delve into the Wheelerian archive that I have been constantly using throughout this dissertation to highlight the tension between "scholarly mentality", and the research work which Wheeler in his imitable style had referred to in his Third Staff Memorandum in August 1944:

> Here I will insert a true story, in the hope that by smiling at ourselves we may begin to take ourselves seriously. At the conclusion of an inspection, I discussed a number of points with the official in charge who, to each of my suggestions, replied emphatically and very properly, ' Sir, I will do it tomorrow'. At last, I came to the question of research. I remarked that although he had been there many years, I could not find that he had produced any evidence of research into the many problems that lay to the hand. His eyes lit up with the fires of eager intent. ' Sir' he said earnestly, 'I will do researches tomorrow.' And there is no doubt that he sincerely meant it (AACD, File No. 33/24/1944).

Although I cannot narrate a similar tale from my ethnographic archive, this "true story" as Wheeler describes, is instructive to understanding the functional role the idea of research and research design had for the ASI archaeologists. It was subsumed under the discourse on scholarly mentality, corruption, and the inner workings of CABA and the institutional hierarchy of the ASI – each member of the ASI staff member had sufficient agency to allocate to other member of the organization – rarely did I see anyone take up the responsibility for research. For the ASI excavation team, the research design was primarily "given from above, [*uper se*]" and they were only expected to "work at the bottom [*neeche se*] " The issue of who frames the research question was also a question of professional hierarchy and therefore the responses that I have noted above were so disparate and contradictory. The larger research objectives were defined in abstract terms, in a few terse pages, when the Director of the excavation submitted a proposal for excavation to the CABA. At the level of daily archaeological practice, it was framed by the discoveries in the trenches and the quadrants and the transformation of evidence into systems of representation. These were determined by the cultural history archaeological methods and process followed by the ASI teams which was no different from that advocated by Wheeler more than fifty years ago.

The Excavation Report

The Excavation Report was the final epistemological product of an ASI archaeological

excavation. It was a formidable piece of representational text that was considered both academically fundamental and obligatory for public consumption. The ASI regarded the excavation report as the essential intellectual result of a scholarly pursuit and its importance was also framed in terms of the moral responsibility of a statist institution to provide information to the public. The Parliament Committee on the functioning of the ASI, headed by Nilotpal Basu, observed:

> The Committee is of the view that if excavation reports are not written, then all the taxpayers' money spent on them goes to waste. Also, in absence of the timely publication of excavation reports, the achievements of the Archaeological Survey of India largely remain unnoticed by the public (Basu 2005).

An AA in Hansi persuasively elaborated on the above rationale, which was also the dominant perspective in the ASI: "after all, we are spending public money. If we do not publish it would be a criminal waste of these resources. The public has the right to know what we have excavated and recovered. We are doing all this for the national good. If the public does not know what we have dug up, then the government should stop all this excavation business. The ASI should then just concentrate on preserving monuments and heritage. We should stop digging. We should cease to be a research institute and become only a heritage management organization." This harsh opinion was widely heard especially during the years I was doing my fieldwork, because it was then that the issue of unpublished excavation reports was being hotly discussed by the archaeologists in the ASI. The 2001 Lal Review Committee Report had also noted that unpublished reports of major excavations were a big embarrassment to the ASI. It gently admonished: "It is acknowledged on all hands that excavation is nothing but destruction if it is not published in detail. It is just not enough to publish brief notes here and there" (Lal 2001: 184) – referring to the mandatory synopsis of excavations published in Indian Archaeology - A Review (the Annual Publication on ASI excavation reports). These were very short description of all the excavations conducted in India over the course of a calendar year.

It was common knowledge in the ASI community that there was a huge gap between actual excavation and publication of excavation reports – one famous example that would be often cited during my conversations was the report of Kalibangan published nearly 33 years after the excavation. B.B. Lal, one of the excavators and editors of the report, wrote with anguish in the preface: "In presenting this report to readers, so many years after the excavations, we feel

really small, nay even ashamed. The only umbrella under which we can cover our heads is the saying 'Better late than never.' We hope that the academic community will accept this much-belated report for whatever it is worth." (Lal 2003: v). I was often told that the Dholavira report would also meet with a similar fate. An AA at Dholavira sadly noted: "I am not even sure if the Dholavira report will ever be written. After more than a dozen years of excavation, we only have some articles by Bisht sir, and the insignificant mentions in the Annual Review – which is also delayed by more than five years. Now Bisht sir is retiring in less than six months - he will also have similar problems like Lal sir. Before he retires, he should at least start the Dholavira Cell to begin the process of collecting and assembling the excavation material and artifacts. It will be a monumental miscarriage of responsibility if the Dholavira report is not published. But the way things are going, I will be surprised if it is ever published." Such concerns were not unwarranted, as most people I asked about the status of the Dholavira report would wave their hands dismissively. A photographer wryly commented, "only God knows if the report will ever be published. I really hope Bisht lives for another 20-30 years - only then we will see the face of the report."

The status of unpublished reports in the ASI was a peculiar postcolonial problem. Although Wheeler mentions the importance of publication numerous times in his missives to ASI officers in Ancient India and the Staff Memorandums (see AACD, File No. 33/24/1944), unpublished reports were not considered a pressing problem then. However, it was Wheeler who was the earliest to notice that unpublished reports of the ASI had become a problem in postcolonial India. The first post-independence review of the ASI in 1965, headed by Wheeler, more than twenty-five years after the damning Wooley Report, mentioned:

> During the past ten years, no fewer than 64 sites have been thus examined, and in March 1965, eight sites are actually under investigation. The total effort, in quality as in quantity, is impressive; as a result of it, we now have appreciably more information about several aspects of Indian culture than we had ten years ago...More serious is the accumulation of un-published excavations; at present moment no fewer than 14 excavation-reports are outstanding, some of them (including one of the most important) going back to 1955. This is wrong, whatever the cause. It cannot be too often emphasized that unpublishable excavation is destruction, and is therefore wholly unjustified [emphasis in the original] (Wheeler 1972: 16-17)

Although delay in the publication of excavation report was a norm in archaeology throughout

the world, it had taken a serious proportion in ASI, noted by all the actors involved in India. In 1984, the Ram Niwas Mirdha committee report also observes a similarly dismal situation, but it was only after the 2001 Lal committee report that it became obvious that the non-publication of the excavations report had reached epidemic proportions. By 2005, more than three Parliamentary Committees had observed the gravity of the state of affairs. The 49th Parliamentary Committee Report on the Demands for Grant (2001-02) of Department of Culture explicitly stated without mincing any words, that the "practice of excavating something and not publishing reports had become a chronic malaise." The 56th Report on the Demands for Grant (2002-03) of the Department of Culture caustically observed that it was "aghast to find that final reports on excavation works have been pending for more than fifty years. Some of the excavation work had been taken up during 1947...The Committee feels the Department was having a lackadaisical approach in the matter." By 2005, the time of the Nilotpal Basu Parliament Committee, non-publication of reports had become a political matter and was reported with alarm:

> The Committee had, in the last few years' reports on Demands for Grants of Ministry of Culture, repeatedly taken up the matter relating to the non-writing/publication of excavation reports by the Archaeological Survey of India. However, inspite of repeated recommendations by the Committee for early completion of the excavation reports, the situation had not improved much. In the status note submitted to the Committee, the Committee was informed that the ASI has carried out 292 excavations after Independence. Mandatory reports thereof have been published up to 1998-99 in the Indian Archaeology – A Review (IAR), an annual bulletin of the Archaeological Survey of India. The remaining issues are in various stages of compilation, editing, and publication. Out of 292 excavations, 102 had been identified as requiring detailed reports, of which 45 excavations reports have been published, one is in press, while 56 are pending for detailed report writing... out of 56 excavations, 17 belong to the category of
>
> large-scale excavation and 39 to the small-scale excavation. (Basu 2005: 14).

The staggering magnitude of non-published excavation reports by the ASI had became a common topic of discussion and conversations by the time I was doing my ethnography. Local and national media were also publishing reports about this situation. A reporter of The Hindu scathingly reported: "While there is a fierce debate going on about history being rewritten in textbooks, the Archaeological Survey of India (ASI) is making sure that it stays away from

any controversy and does not get any history 'written'. Literally" (Nayar 2004).

For those in the ASI, this was a matter of embarrassment as illustrated by B.B. Lal's apology in the Kalibangan report cited above, but none were able to adequately pinpoint the cause of the delay. But there were various theories regarding the non-publication of excavation reports and everyone contributed their insights. The co-Director of the Dholavira excavation patiently explained to me "there is no point in blaming excavators for not writing the report. It is the system. The government expects us to be both academics and bureaucrats. They want us to painstakingly dig a site, and then go back to the office to sit and write an excavation report right away. But when we reach office, we get drowned with so much bureaucratic work [*babugiri*] that there is no room to breathe - files, redundant paperwork, court cases, financial accounts, budgets, parliament questions, hundred of pointless queries from the ministry or the DG office. It is just back-breaking." True enough, when I first met this officer in the DG office, in Delhi to get permission to work in Dholavira, he had asked me after a cup of tea if it was possible for him to get a grant to work in an American university to "peacefully [*itminan*] write an excavation report". Later on in the field, he further explained as we were sitting in his boonga in Dholavira, one evening after dinner "those excavators who are interested in writing reports are unwarrantedly transferred, and the system makes all attempts to ensure that they cannot write their reports. Tell me how can one write a report, if the antiquities of the site are all locked up in Delhi, all the trench note books, the site drawings, and photographs are in Nagpur and you are sitting in Agra trying to write a report of a site that is in Gujarat. It is simply absurd [*pagalpan*]. Can you imagine how much paperwork and time it takes just to get access to a site notebook from an office where you are no longer the boss?" The excuse of ineffective bureaucratic machinery was not novel; it had already been brought to light by the Lal Committee report, which pointed out that one of the major reasons for the non-publication of the excavation reports were the indiscriminate transfers of officers in charge of excavations, which did not give them time to finish writing the reports: "the shifting of excavators from one office to another, without seeing to it that the reports are completed before the shifting, has resulted in a major backlog" (Lal 2001: 184).

The issue of indiscriminate transfers and its effect on the non-publication of the excavation reports was indeed a real problem and the blame for this was put on the Administration department of the ASI by almost all ASI officers. They believed that the ASI Administration

under IAS officers was the major cause of the mess that the ASI had gotten into. A DySA in Baror explained " these administrators [*babus*] are not archaeologists and are not sensitive to needs in the field and neither are they aware how archaeology works. They are basically administrators – they are good at babugiri, and they do not understand that archaeology is not just about digging holes but also about writing, which is an extremely important and time consuming process. So they cannot understand how harmful indiscriminate transfers are." The Niloptal Basu Parliamentary Committee also noted that the untimely transfer of excavation Directors caused significant delays in the writing of the report:

> The Committee recommends that the Archaeological Survey of India should enforce a fixed time frame for writing excavation reports after the excavation work has been completed, so that the public at large is not deprived of the vital right to information. The Committee also recommends that the ASI should ensure that officials engaged in a particular excavation work are not transferred till such time that they complete the excavations and submit their reports (Basu 2005: 16).

The official explanations I got squarely placed the blame on the "system" and the "administration" - the favorite flogging horse. In the narratives I heard, it was common to alliocate agency to the postcolonial bureaucratic system for the present state of the ASI and I believe this was very effective as it allowed the victim to be a Kafkaesque casualty of the "system."

However it is the theories and explanations that I heard from the lower level of ASI hierarchy that I found most intriguing. These were critical of the official narratives that senior officers gave to me. A draughtsman in Bhirrana noted: "For them [the excavators], digging is just another routine-work. They dig because they are supposed to dig. Report writing is a different issue and most of these senior officers just don't have the patience to sit, think, and write." Similarly, an ASA in Baror explained, "First, most of these excavators are not academic. They have PhDs but their mentality is that of an administrator [*babu*]. They are happier signing infinite files rather than sitting and writing something intellectually worthwhile. Also, you have to understand excavation is now just a means of making money. As you know, corruption is rampant. Everyone wants to make more money than he has, and for those who run the excavation branches, there is no other way than digging sites. So what they want is to dig sites so that they can show in their annual expenditure budgets that they have spent the entire allocated amount for excavations. Writing reports is obviously not so profitable. So

there is no inclination for doing that." This explanation was both logical and plausible. The archaeological process that I have described in the chapters above was motivated more by the need to expose a large archaeological landscape than by a coherent research program. Excavations were more a bureaucratic act than a knowledge driven project. On this prioritization of activities, excavation reports did take a back seat.

However, in the long list of unpublished reports, there was an exception. It was the report of the Ayodhya excavation. The 2003 Ayodhya Excavation Report was a record of sorts; something that Dilip Chakrabarti also acknowledges in an op-ed written in the *Hindustan Times* entitled "It's archaeology, stupid!" He notes with mock jubilation and a shade of cynicism:

> Considering that only 15 per cent of all the archaeological excavations undertaken in India since Independence are properly published, the submission of a full report on any excavated site in the country should be a matter of great rejoicing among archaeologists. When I read on the internet that the ASI had submitted its report on its five-month long excavations at the site of the now-demolished Babri mosque in the early historic city site of Ayodhya, my initial reaction was that of joy. I was also happy because here was at least one case when the ASI could prove its mettle. Its officers could come up with a full report within three weeks of the completion of a full excavation season. Whoever heard of such a thing in Indian archaeology? Could there be more of archaeology under the judiciary in our country please? (Chakrabarti 2003b).

The high profile Ayodhya excavation was conducted under the watchful eyes of the Indian Judiciary – the Lucknow Bench of the Allahabad High Court, and the excavation report was submitted to the Court in August 2003 - less than two months after the excavation. This report has not been published but it is still considered to the fastest excavation report ever written by the ASI. Written under huge political and judicial duress, as a number of my informants who were involved in the Ayodhya excavation and the writing of the report told me, it was a veritable feat. It was also a factual contradiction of the reasons for delays in excavation reports cited by the ASI to the Niloptal Basu Parliamentary Committee, which stated:

> Regarding delay in writing the excavation reports, the Archaeological Survey of India informed that the delay is due to their nature and scope, as these are supposed to be comprehensive research documents, containing information on excavated structures,

> stratigraphical features, supported by maps, illustrations – photo-documentation and line-drawings of excavated structures, potteries, artifacts, and other materials. The report also would have to incorporate detailed technical and scientific reports and bibliographical references. Since a huge body of data is generated in course of excavation, it takes time to organize and describe that exhaustive database on factual and interpretative lines. For report writing, a team of excavators, who participated in that very excavation, is required to discuss, prepare research notes, and correlate the excavation findings for description in terms of time and space. This is true in the case of all excavations (Basu 2005: 15).

An AA in Baror who had been involved in the excavation and the report writing at Ayodhya told me "if the ASI can produce an excavation report on Ayodhya, it can do so for any site in India. The Ayodhya report came out because of the court's pressure [*court ka danda*]. If there is pressure like that, all the excavation reports will be published in less than a year. It is just a matter of discipline, which is lacking in the ASI."

The unpublished excavation reports of the ASI were an illustration of the broader practice of postcolonial archaeology in India. The excavation report in archaeology is not just the final epistemological act of an archaeological event, but it symbolizes the fruition of the archaeological project. Its publication signified that the excavators were willing to share their insights with the scholarly world and the larger public. The ASI was aware of this symbolic importance of the excavation report and therefore its non-publication was an embarrassment to the organization. Somewhere – jostling between systemic incompetence and individual weariness – was a failure; and everyone had someone or something they could blame for it.

Conclusion

The critical focus of this chapter was on the performative and representative practices of the ASI archaeological excavation, and specifically their processes of enunciation. The ASI was deeply driven by the need to present and represent archaeological excavation as a scientific enterprise, to justify its large-scale intervention. The site visit and its lengthy preparation revealed this subtext of justification and alluded to a professional insecurity that archaeologists in the ASI experienced in comparison to other governmental institutions working in the service of the nation and state – army, district administration, and politicians. I have tried to argue in this chapter that the ASI archaeological intervention was structured by

its need to showcase its credibility as a scientific and bureaucratic organization of the state. This logic not only drove the ASI's performative and representation strategies but it also framed their theory of archaeology. The ASI, being aware of the ephemeral and destructive nature of archaeological knowledge, believed in the need to represent archaeological evidence in a standard lexicon. It spent a significant amount of time and energy focusing on presentation techniques and subject preparation. Both these practices of performance and representation were driven by a fetishized obsession with presenting the evidence produced at the site via a colonial vocabulary, as I have shown through my discussion of photography. The unpublished reports of the ASI epitomized a "chronic malaise," which now have become a part of the public debate. As I have attempted to demonstrate throughout this dissertation that the archaeological practice that ASI performed was Wheelerian at its best and fragmented postcolonial malaise at its worst.

Chapter 7
Conclusion

Ethnography, with my specific focus on the ASI, functions as a detailed study on the epistemological practices of archaeological science in the postcolony. As such, the aim of this dissertation has been to demonstrate that archaeology as an epistemological practice is principally a cultural practice arbitrated by the modernistic rhetoric of its epistemic application. The authority of its claims does not derive from the scientific method of its practices as claimed by the processual archaeologists, nor does it derive from the interpretative nature of its knowledge construction processes, as argued by the post processual archaeologists. Archaeology is not a practice subverted by political and nationalistic goals of the location of its performance. Instead, I argue that archaeology is itself an ideological and cultural practice; the boundaries between construction of scientific evidence and its ideological or interpretative creation of knowledge are non-existent. The epistemological process of archaeology is itself a subjective and heterogeneous practice that employs science as a methodological rhetoric and eventually applies interpretative techniques to create a narrative about past.

Through a detailed study of archaeology's trajectory in a marginal location of the postcolony – an idiosyncratic site in comparison to the more stable location of the Euro-American world, I demonstrate the epistemological fallibility of its practice. I emphasize that in the context of the processual and post processual debate it is necessary to investigate the gulf between theory and practice by examining archaeological practice outside the Euro-American location. I illustrate that archaeology in postcolonial India, although applied as a Western practice, has evolved into a distinctive process of knowledge construction - an epistemological vestige of its colonial genealogy embedded in the structural framework of postcolonial governmentality. Archaeological practice in India is an articulation of a colonial ideology performed in the postcolonial landscape - from the procedure of the excavation to the processes through which laborers are employed and controlled in the excavation site. The objective power of the ASI emerges from both its roles as an institutional formation of postcolonial governmentality and the fractured science of its archaeology. By focusing on such a fractured location of knowledge production, I investigate the epistemological subtext of the discursive practice of

archaeology as a science—all the while demonstrating its ideological basis.

Archaeology - a bureaucratic science?

When I conceived of this project, I had framed it as an ethnographic investigation of archaeology as a scientific enterprise in postcolonial India. I was of the belief that culture-history archaeology in postcolonial India was a rich site to examine the ideological and philosophical practice of archaeology as a science. I had chosen postcolonial India because I was particularly interested in investigating how archaeology functions in a non-western setting in order to contribute to the larger discussions on theoretical archaeology. I believed that it was imperative to shift the geographical focus of the debates in archaeology fixated on the Euro-American world and to bring new insights into the deliberation on theory and practice of archaeology from a different geopolitical universe. In this context, the ASI, with its extraordinary history of archaeological intervention, its deep colonial legacy, and its overwhelming presence in contemporary India, became an obvious site for this examination. My decision was also driven by the fact that the ASI was not just one of the oldest archaeological organizations in the world, but it was also one of the largest of its kind holding hegemonic control over the Indian nation's archaeological heritage. I regarded the ASI as an iconic location to not just comprehend the discursive practice of archaeology as a postcolonial science, but also as a productive site to investigate the theoretical and methodological issues that archaeology,as a modernist discipline, was struggling with.

I theoretically framed my work as an attempt to study the practice of science in archaeology. Although there has been considerable theorizing about the interpretative approaches in archaeological reasoning, only recently have archaeologists sought to theorize the practice of archaeology. Prior to these attempts, archaeological practice was subsumed under the pedagogical rhetoric of method. Since the classic culture-history field-method textbooks by Petrie, Wooley, Wheeler, and others, practice has been discussed in archaeological discourse as a didactic method to be followed. With the rise of processual archaeology, methods of archaeology have become increasingly a process of infusion. In a fundamentally cultural-historical practice, laboratory approaches, borrowed from adjoining disciplines, were introduced. Botany, zoology, geomorphology, geography, chemistry, and statistics were the prominent disciplines for importing methods. The debates of the 1980-90s were centered on the theory of archaeology and a disconnect with practice was obvious in these discussions. It

was with reflexive archaeology's theoretical realization about the importance of investigating practice that a shift was noticeable.

Methodologically, this dissertation has been influenced by a series of ethnographies of archaeology that were written in late 1990s & early 2000s - most of these works emerging from the theoretical framework of post-processual archaeology and its reflexive incarnation (Hodder 2000; Bergren 2001, 2003; Hodder & Bergren 2003). Some of these ethnographies attempt to interrogate the impact of archaeological intervention in the local community, as there has been widespread credence to the theory that archaeological intervention, especially conducted by archaeologists from North in sites in the South, was an ideological continuation of the colonial project (Castanda 1996). Other ethnographies have been influenced by burgeoning scholarship in the area of the sociology of science investigating the epistemological foundation of archaeological work (Edgeworth 2003, 2006; Bradley 2003; Holtorf 2002; Yarrow 2003). These were attempts at contributing to a growing literature in post-processual archaeology with a theoretical emphasis on archaeological practice (Hodder 1999; Lucas 2001). My emphasis in this dissertation is to bring together these multiple discursive strands in order to understand the highly fractured location of archaeology, as a discipline, not only in India but also throughout the world, as it continued to come to terms with its disciplinary subjectivity. The rancorous friction between processual and post processual, the increasing instrumental power of archaeology in the creation of nationalist, indigenous, and local identities made archaeology a dynamic location to investigate the ideological, methodological and theoretical constitution of its practice.

In my work, I was compelled to combine these varying discursive and methodological strategies. Archaeology in postcolonial India, I recognized, was both an extension of the colonial project and an epistemologically driven scientific practice. This was further aggravated by the extraordinary intersection of nationalism and politics that governed archaeology's epistemological trajectory – in terms of the objective claims it made and the authority it excreted. Undoubtedly, the genesis of this project was also situated at a high moment of the collusion between archaeology and nationalism during the destruction of the Babri Masjid in 1992. I was personally affected by the communal riots that followed in Bombay in the December of 1992 and January of 1993, when more than 2000 people were killed. For more than six months after communal riots, I worked as an activist with an NGO

in Bombay assisting the victims of violence in procuring relief from the state and helping them in rehabilitation and restitution. This violence had a widespread impact in the contemporary polity of Indian national life in the 1990s through mid 2000s. The strengthening of Hindu fundamentalism and its influence on archaeology, although prevalent throughout its colonial history, was aggravated during the demolition of the Babri Masjid. However, this relation entered the international arena during WAC 3 in New Delhi, where the stranglehold of Hindutva ideology within the top echelons of the archaeological community in India was unambiguously displayed to the world archaeologist community. Furthermore, the excavations *only* by the ASI at the disputed site of Ram-Janambhoomi and Babri Masjid, ordered by the Lucknow Bench of the Allahabad High Court in the summer of 2003, increased the necessity for conducting this ethnography. The astonishing nature of public fascination and the profound juridical importance attached to the evidence produced during the excavation further influenced the theoretical framework of this dissertation. It was these incidents that led me to conjecture in my proposal that interlinking connections—colonial, scientific, theoretical, methodological, epistemological and nationalist—made postcolonial India a volatile, but fertile site to examine archaeology in action.

By using the theoretical and the methodological approach of a sociology of science and the critical analytical framework of postcolonial studies, I attempted to structure my ethnographic intervention as a postcolonial critique of science and archaeology in times of ultra-nationalism. Thus, the choice of studying the ASI's archaeological intervention rather than a university excavation or excavations conducted by the archaeological organizations of a provincial state, was obvious. The umbilical link of the ASI to a nationalist and postcolonial India was ubiquitous. The enormity of authority this organization wielded in India made it a perfect site to study the discursive intersection of archaeology, science and postcolonial nationalism.

However, when I began my fieldwork in the summer of 2003, I rapidly observed that archaeology in the ASI was not *overtly* about nationalism or patriotism as I had assumed. Some of my informants suggested time and again to me—from senior officers to laborers—that for them, the practice of "doing" archaeology was an act of patriotism. But I observed that the rhetoric of nationalism was just the superficial verbalization of a routine and bureaucratic work. For the ASI archaeologists and other employees, the 1992 demolition of

Babri Masjid and the excavations at Ayodhya in 2003 were only irritants in the routine work of the state. These incidents impeded their normal customary schedule of managing, excavating, and preserving the rich archaeological heritage of India. They viewed the 1992 demolition as a problem of law and order, while the Ayodhya excavation of 2003 appeared as an incident of governmental duty exercised under the pressure of the court. The longer I spent time at the excavation sites observing archaeological practice of the ASI, I came to another realization: ASI archaeology is not an *overt* articulation of a scientific practice; it is a "craft". This craft itself is a science–a learned trade, a practice inherited from seniors, a continuation of tradition imbibed in a field school (Shanks & McGuire 1996; Walker & Saitta 2002)—which, if applied as directed and learned, is expected to generate archeological evidence. This practice of archaeology as a science was theoretically located in the old-fashioned cultural-historical mode, carried out as an outdated but symbolical and functionally overwhelming Wheelerian methodological practice. However, it had a powerful objective valence within the imagination of the Indian scientific community. The scientific practice of the ASI was so influential that in 1989, the ASI was declared as a Scientific and Technological Department of the Government of India (Basu 2005). But during my ethnography I realized that the ASI did not derive its authority and power as a knowledge production organization; it acquired and excreted its influence as a governmental bureaucracy. Although I was aware of the statist nature of ASI, I was not prepared for the engulfing presence of bureaucracy.

As I immersed myself in the ASI camps and sites, I was overwhelmed by the powerful governmental and bureaucratic impact of ASI as a statist organization. I had begun my project searching for archaeological practice but was overcome by the daily practice of bureaucratic governmentality of the ASI. Archaeology in the ASI operated with the logic of any other practice of postcolonial governmentality. It was plagued by sluggish bureaucratic machinery and an unyielding institutional organization, colonial in its conception and steeped in inflexible hierarchy. Rampant corruption and its epistemological functions (conducting excavations, publication of reports) were articulated as an apathetic apparatus of postcolonial governmentality. This indifferent postcolonial governmental instrumentality was apparent not only in the way that the ASI interacted with its subject, but also in the way in which the organization's institutional bureaucracy objectified its own employees. I soon realized that the knowledge production enterprise of the ASI in the excavation site was a mere by-product of its governmentality rather than solely the main activity from which the ASI derives its authority

and status. Archaeology in the postcolony is an overt instantiation of the statist bureaucratic apparatus rather than a nationalist or a scientific act. Postcolonial archaeology is archaeology as bureaucratic practice.

Furthermore, I observed that the daily practice of archaeology onsite in the trenches was not at all driven by the nationalistic ideological structure as I had assumed when I was writing my proposal. Although nationalistic narratives of Indus or Saraswati populated the discursive space of archaeology, at the level of practice, such nationalistic subtext was absent at the sites even in such highly politicized projects like the SHP. I observed that the day-to-day practice of archaeology in the ASI site was conducted as a bureaucratic procedure which employed science as a functional apparatus that had the capability of generating empirical evidence about the past (antiquities, monuments and material culture). The daily practice of archaeology was mediated by the scientific logic of culture-history archaeology. As a matter of fact, my observation and arguments would have not been any different if I had worked at sites not attached to the SHP. National and ultra-national narratives about the past, I observed, were not framed or even expressed during the daily practice of archaeology in the trenches, which was viewed as primarily an act of evidence gathering. The nationalistic narratives emerged only when interpretative leaps of imagination occurred. It is important to emphasize here that the theoretical and methodological influence of processual and post-processual archaeology was minimal if not completely non-existent.

After my fieldwork, when I began writing this thesis, I had to reframe the larger meta-theoretical structure of my project. I had to considerably downplay the importance of nationalism in the daily practice of archaeology and underscore the bureaucratic nature of archaeological intervention in postcolonial India. This dissertation, which at the stage of its proposal, was about archaeological science within postcolonial nationalism, was now transformed as a project interrogating postcolonial archaeology as a bureaucratic science. Thus, as I have stated in the introduction to this dissertation - this ethnography is as much about archaeology as it is about the state - it is as much about science as it is about the postcolony. The ASI's archaeology as bureaucratic practice represents an idiosyncratic setting of archaeological knowledge production and it is imperative to study such a peculiar site to examine how archaeology functions as science. In this framework, I believe that the idea to subsume archaeology and the postcolonial state under the rubric of marginal modernity is a

productive theoretical ambit to comprehend the way these notions perform in a spatiality that i itself personifies marginality – rural India.

ASI archaeology was driven more by the thrust of postcolonial governmentality than the desire to produce knowledge - although the eminence of knowledge has been employed to increase its authority about Indian past. Science and its rhetoric play a central role in emphasizing the influence of the ASI. The scientific practice of archaeology is subverted and exploited by the governmentality of ASI to essentialize its objective authority over Indian past. ASI archaeology has not evolved from the days of Wheeler. It is still articulated as a colonial project, albeit with the government logic of postcolonialism. Although science in the ASI archaeology is craft, it has a powerful objective valence. The craft is viewed as the most efficient and ideologically objective practice in the production of knowledge. However as I have tried to show in this dissertation, this craft of science is itself an ideologically driven practice – bureaucratic, panoptical, oppressive, and exploitative.

The objective valence that has been given to this craft is itself unfounded. Science in archaeology is a social practice and the knowledge that archaeology as a science produces is not without its ideological and cultural subtext. ASI archaeology assumes that this craft of scientific practice, if applied arbitrarily, is methodologically robust enough to produce impartial knowledge. The ASI believes that a systematic articulation of this craft of science, a concept essentially Wheelerian in nature, is the appropriate technology in the production of objective knowledge about the past. As I have argued in my early chapters, the choice of Wheelerian archaeology was not because the ASI was caught in a time warp. Instead, Wheelerian archaeology has been preferred as it is an archaeological practice that not only is productive in the generation of scientific evidence about the past, but it is essentially a bureaucratic practice that was also designed to control the personnel working at the site. The science of archaeology is thus transformed into a performative and rhetoric practice, which is exploited politically to create narratives that justifies its political leaning. It is this belief in the objective efficacy of archaeology as a scientific craft that gives archaeology, and especially ASI archaeology, in times of ultra-nationalism and statist influence, unparalleled authority in postcolonial India.

Postcolonial Oppression

The ASI, in this dissertation, is employed as a metaphor for postcolonial governmentality. It is a representative institution of the postcolonial state involved in the daily practice of management, organization, and knowledge production about its subject and territory. Cohen has argued that organizations like the ASI represented the 'historical modality' and the 'survey modality' of the colonial state. However in the postcolony, the ASI was created as an instrument of governmentality. Its birth lay in the colonial state's need to explore and produce knowledge for governance, but its postcolonial avatar was a managerial and governmental edifice. Its knowledge production ability, although overtly flaunted and celebrated as indispensable for its professional subjectivity, was subverted and subsumed by the statist apparatus of governmentality.

The ASI was a routine statist organization involved in the standard task of governance and a postcolonial site of relative calm. Unlike the governmental apparatus working in sites of extraordinary tension and contrast, where the postcolonial state exhibited its brute power – zones of military occupation in insurgency effected areas in the north east India, Kashmir or Punjab, where extra-judicial killing was the norm; or regions of perpetual administrative emergency in tribal belts of central India where Maoist extremists were eliminated with ruthless aggression; or sites of development projects where forcible removal of people without due compensation was customary. The ASI archaeological site was nothing like these zones of palpable conflict. It was a staid location of the articulation of statist authority. Here, through the daily practice of routine work, the state machinery directed its institutional force in controlling and managing the lives of people under its jurisdiction - from the senior officers to the local laborers. ASI archaeological operations were highly codified and hierarchical processes of knowledge production, and worked within the logic of industrial time and military discipline. This was primarily a disciplinarian practice and within its structural formation, oppression was intrinsic. The power relation between the state and the subaltern subjects, who formed the base of any archaeological excavation that the ASI undertook, was no different from the militarized, colonial organization it had been before independence.

The hierarchical nature of ASI archaeology; the interventionist mechanism of its statist organization, as it penetrated the peripheral territory in the margins of India; the way it transformed a rural landscape into state-occupied territory or as epistemological sites was not

only located in the colonial genealogy of the ASI, but was carried out as a colonial project and conceived as a scientific project – intrinsic to all these was systemic oppression. Postcolonial archaeology was a mix of Wheelerian methodological practice and pre-Wheelerian colonial archaeology, which employed huge amounts of labor to virtually strip the earth to uncover an ancient civilization. The civilizational subtext of the ASI archaeology was methodologically fixated more with monuments than with material culture, more with antiquity than with artifacts. During the course of my fieldwork, I realized that archaeological excavations of the ASI were not just a postcolonial instantiation of Wheelerian archaeology. They are instead, I contend, a representation of large-scale *colonial archaeology* at work with its apparatus of oppression intact, if not further sharpened. This was obvious in the way that the excavation process was imagined and carried out - the acquisition of land, the setting up of the camp, the exploitation of local underprivileged people as laborers. In this intervention, the colonial natives were viewed as postcolonial illiterates and brown sahibs substituted the white sahibs. An old activist song that I learned long ago while working in the tribal areas of western India succinctly described this postcolonial predicament: "...the white rulers have gone, but the black masters have come instead. The keys have changed but the locks remain the same" [*...gore haakim gayo re bhaiya, aageya haakim kale. Badel gaei hai chabi lekin badle nahi hai taale*]. If archaeology as a discipline is in the state of subjective crisis, then Indian archaeology is in a state of complete distress. I have maintained in this dissertation that this is due to the marginal status that archaeology and the postcolony have in modernity, exacerbated by the sociological and cultural subtext of archaeology as a science in general. Through this dissertation, I provide a critique of such a postcolonial means of knowledge production. I argue that the appalling state of Indian archaeology is simultaneously a product of the objective status of science in archaeology and the uniform degree of statist oppression that is prevalent in the postcolonial bureaucracy.

Scholars and theorists have discussed postcolonial ambivalence and postcolonial hybridity. I would not put forward a theory of ambivalence of any kind in the postcolony. The ASI, as an institution of governmentality, is a not any ambivalent location of hybridity. It is a site of systemic oppression, which is uniform and impacts all. In such a context, postcolonial critical theory is an oxymoron. It is a theory of the colony and colonial engagement of the metropole articulated by scholars who inhabited the postcolonial temporality. The *post-* in postcolonial theory is only the temporal location of the scholars who theorized the colony. It is not about

the postcolony. Thus, probably the colonial encounter was indeed a moment of ambivalent and hybrid encounters, but in the postcolony, such ambivalence and hybridity are at best marginal. The postcolony, especially as a location which saw the brunt of statist governmentality even in such a routine site as an ASI excavation site, is an oppressive location of power.

Archaeology - a homogenous discipline?

The ASI excavation that I have described in this dissertation can be argued, in relation to the contemporary advances in archaeological methodology, to be a distorted form of archaeology. A case could be made that archaeology, as a bureaucratic practice in postcolonial India, is all that archaeology is not about. This practice of knowledge production undoubtedly epitomizes marginal modernity, but to ridicule it as bad archaeology would be to miss the point. ASI archaeology is a science as viewed by both its practitioner and those who consume it in India; to relegate it to the universe of bad practice would be to disparage the epistemological authority that it wields in the fractured historical and political space of postcolonial India. I would like to stay away from such a criticism of the archaeology that I have described above. In this dissertation, I have tried to demonstrate and argue that postcolonial archaeology is how archaeology as a marginal science functions in the periphery of modernity. Archaeology, like any scientific practice, is essentially a social and cultural practice heavily entrenched in the cultural and political structures of the location it functions in. Archaeology in the postcolony similarly is a product of the social cultural aspects of its location. My ethnographic focus on daily practice has been to emphasize that even in an ideologically motivated space like the ASI, archaeology works as a science – albeit a fractured science.

Archaeology unlike any other discipline in the social sciences or humanities, is the most hybrid – in theory, in practice, and in the methods that it adopts and articulates. This allows for numerous permutations and combinations of approaches and theories to be passed onto the disciplinarian discourse of archaeology. In India, we see one such combination, embroiled in science, history, nationalism and bureaucracy. To put the blame on the bureaucratic corruption of the postcolonial state for the fate of archaeology in the colony would be incorrect. It is archaeology's theory of practice that makes it susceptible to such idiosyncrasies. It is archaeology's hybrid disciplinary subjectivity that not only allows for such a varied articulation of its practice but it also is responsible for the bitter debates of its theoretical

framework. The theoretical idea that this ethnographic description of archaeology throws up is to disturb the perceived homogeneity of archaeological practice. The belief in the fixity of archaeological practice that has been fed into the disciplinarian discourse of archaeology since the publication of the earliest fieldwork textbooks, is fundamentally challenged in this dissertation. Through the articulation of the idea of archaeology as a bureaucratic practice, I have tried to show the hybrid character of its theory of practice and problematize the assumed homogeneity of archaeological practice.

I contend that the uniformity of archaeological practice, taken for granted by scholars theorizing about archaeological practice, is tenuous. Since the obsession with theory began in archaeology with the rise of processual archaeology, there has been an unquestioned premise about the homogeneity of archaeological practice and method. Other than minute differences between American and European archaeology, it has been largely believed that the practice of knowledge production in archaeology is normative and homogenous. This is largely a product of the fetish with American-European archaeological traditions that have ignored the trajectory of archaeology outside its temporal and geographical ambit and subsume any variation especially in the case of Anglo-American world within its theory and practice. The underlying assumption has been that the core archaeological practice was static and unchanging. The fundamental idea that it is only through digging that the evidential empirics of archaeological knowledge were produced, was never under question other than in discussions centered around archaeology in the philosophy of science. This premise has been an elementary assumption in its scientific practice. The theoretical and meta-theoretical debates about method and theory in past few decades have also inconspicuously assumed such homogeneity of archaeological practice.

Through this dissertation, however, I problematize this basic assumption about the perceived homogeneity of archaeological practice. I demonstrate and argue that archaeology, as a knowledge production discipline with specific acts of practice, is not homogenous, and what we see in postcolonial archaeology is one variant of the heterogeneity of archaeological practice. There has been a dominant influence of Western practice in the making of theory and methodology of archaeology – this dominance has been a fixation, giving rise to the assumption about homogeneity of archaeological practice. Ucko, in 1995, did attempt to break this assumption of homogeneity, but theory-building in archaeology is still constrained by the

American-European practice of archaeology. This is not unusual, as the space devoted to non-western practices in the history of archaeology is very limited and subsumed within the rubric of an extension of the colonial exploits of the metropole. It is because of this geographic and ideological domination that in this dissertation I shift the focus of the location of enquiry from the western world to the postcolony. I contend that in order to meta-theorize the universality of archaeological methods and theory as exemplified in the innumerable pedagogic texts that have been produced since the Petrie's, archaeology needs to re-investigate the practice and nature of knowledge production beyond the West. The impasse between the processual and post processual archaeology that is persistent in the contemporary world of archaeology can gain from this investigation of the heterogeneity of archaeology. This can be fructified in the form of examining the trajectory that archaeology in the colony took and the nature of its practice in the postcolony or in the non-western world.

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